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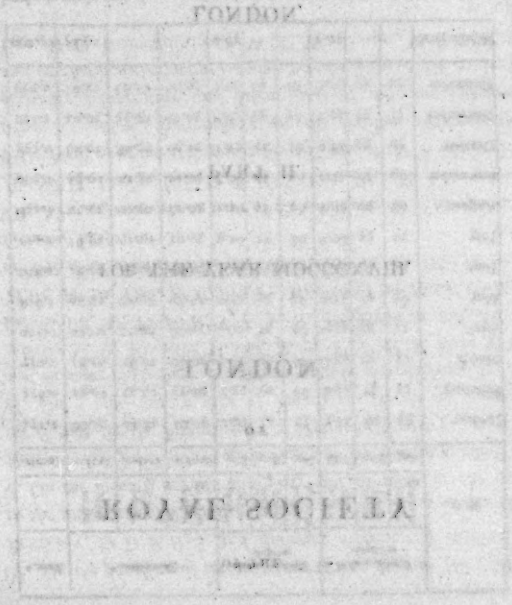

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PHILOSOPHICAL TRANSACTIONS.

XIV. *On the parallax of certain fixed stars. By the Rev.* John Brinkley, *D. D. F. R. S. and Andrews professor of astronomy in the university of Dublin.*

Read March 5, 1818.

THE attention of the Royal Society has been lately called to the subject of the parallax of the fixed stars, by the astronomer royal; and as this has been occasioned principally by the results of observations which I have made at the Observatory of Trinity College, Dublin, I have taken the liberty of offering a few remarks relative to, and connected with this subject.

The Royal Society did me the honour to publish an extract of a letter to the late Dr. MASKELYNE on the parallax of α lyræ; since which time, in pursuing my observations, I have met with apparent motions in several of the fixed stars, the cause of which I was unable to explain, unless by attributing them to parallax. Among these stars α aquilæ exhibited the greatest change of place.

The results of my observations have been published in the 12th volume of the Transactions of the Royal Irish Academy

I there detailed my reasons for supposing that I could not have been misled by any source of error in the instrument, or in the mode of observing; and I trust, whatever may be the final result as to this subject, that I shall not be considered as having too hastily adopted the explanation by parallax.

I remarked, in the essay to which I have referred, my reasons for not being surprised that the Greenwich circle did not immediately confirm my results. However, after several years observations, Mr. POND was led, although he had found discordances of a similar kind, but much less than mine, to doubt the explanation by parallax, and certainly with good reason, as two instruments that might be supposed equally adapted for the examination, seemed to give different results; in consequence of which he took measures for submitting the matter to another kind of trial. He most laudably applied to the Royal Society, as visitors of the Royal Observatory; and by their assistance, and by the advantage of vicinity to the first artists, he has been enabled to put up his fixed telescopes.

Thus, unless unforeseen difficulties shall be found to exist, this question is likely to be soon decided; and certainly on many accounts, it is a most interesting question.

It is now about sixteen months since Mr. POND informed me of his doubts respecting the conclusion I had drawn from my results, and from that time I have anxiously looked to every observation that tended to confirm my conclusion, if just, or invalidate it, if wrong.

The last two years have been very unfavourable for astronomical observations; so that my opportunities always, in consequence of the cloudy atmosphere, very few, have been

during that time fewer than usual. However, I have been able to obtain some results that I shall notice farther on, which appear to coincide with my former ones as to α aquilæ, in a remarkable manner; and it is to this star that we are, I think, to look for the final decision of the question. As to α lyræ and arcturus, my results have not been so uniform as I had expected from my former observations; but as to α cygni, my recent observations are consistent with my former ones, in exhibiting the same discordance between the summer and winter observations as before, which appeared to me to point out a parallax for that star, but less than for any of the other three stars. The results of the observations of Mr. Pond, by the fixed telescope, as given in the last volume of the Transactions, appear, at first sight, very decisive against the existence of any visible parallax in α cygni; but in considering these observations, a difficulty suggests itself, which if founded, will render the result deduced from α cygni and β aurigæ quite inconclusive. This, and some other points relative to the fixed telescopes, will be noticed farther on.

If it shall appear that I have been deceived by a constant source of error in my instrument as to these stars, it will be of much importance to investigate that source; and although at present I can form no conjecture as to any cause, yet, when it shall be found actually to exist, it will be incumbent on me to endeavour most strenuously to investigate the cause; and in so doing, I conceive I shall render a most acceptable service to astronomers. It will be shown, whether it be a cause that will be likely to affect other instruments. It appears at present, from the results of Mr. Pond, that the

Greenwich circle is subject to a similar cause of error (supposing the discordance should not arise from parallax), and that this cause has been diminished, if not entirely done away, by reducing the internal air to the same temperature as the external.

However, from all the consideration that I have been enabled to give the subject, I am led to entertain doubts of the fitness of an instrument similar to the Greenwich mural circle, for this delicate enquiry. I do not allude to the objection stated by Mr. POND, since, as he justly observes, that is obviated by keeping the telescope fixed to the same place on the circle during a period of observations, as was the case in the observations of 1813, and as to those mentioned in the Appendix. And in respect to Mr. POND's paper, and its Appendix, as given in the first part of the Transactions for 1817, it appears to me doubtful, whether the results, if they could be exactly obtained (that is, if the elements from which they are deduced were exact), may not be such as to furnish a discordance explained by a parallax nearly equal to mine, or whether the results might not be entirely against parallax. My reasons for entertaining these doubts, will appear in the following remarks respecting the elements used in computing the index error, in instruments similar to the Greenwich mural circle.

The polar distance of a star, as observed by a mural circle, requires, besides the corrections for refraction, aberration, annual variation, &c. also the application of the index error.

This index error is determined by the mean of results deduced from observations of stars of the standard catalogue.

Let $i =$ index error.

$d =$ mean polar distance of a star of the standard catalogue deduced from observation.

$c =$ mean polar distance of the same star in the catalogue.

Then

$$i = d - c$$

Let $o =$ observed polar distance.

$r =$ refraction.

$p =$ parallax.

$a =$ aberration of light.

$n =$ nutation.

$s =$ semiannual equation.

$v =$ annual variation.

Then $i = o + r + p + a + n + s + v - c$

these quantities being applied with proper signs.

Now i partakes of the error or uncertainty of each of these quantities.

1. Let us suppose that there is no error from the observation or construction of the instrument; that is, let us suppose o exact.

2. As to refraction. Any uncertainty in the quantity of refraction affects the index error, and therefore the required polar distance of a star, although that star should be in or near the zenith. Thus the determination of the polar distance of a star in the zenith, will partake of any uncertainty in the refraction of the lower stars used for the index error.

Let us see to what this may amount as to the index error by a single star.

BRADLEY's refractions, by which the Greenwich observa-

tions have hitherto been computed, differ from the French refractions as follows.

The French refraction at $45° = 57,''5$ } therm. 50, and
BRADLEY's refraction at $45 = 56, 9$ } barom. 29,60
and the mean diff. $= 0'',6 \times$ tan. zen. dist. nearly.
But this will not affect the index error, as it equally affects r, &c.

But the effects of the change of temperature as computed by BRADLEY's, and by the French refractions, have an important concern in this enquiry.

To deduce the actual refraction from the mean refraction for height of thermometer $= t$, the mean refraction is multiplied by $\frac{400}{350+t}$ according to BRADLEY.

According to the French tables, the multiplier is $\frac{500}{450+t}$ at least sufficiently nearly so for the 30 standard stars of Mr. POND.

The difference then between BRADLEY's refraction and the French refraction from the change of temperature, is nearly $\frac{t-50}{2000} \times$ mean refraction.

Now Procyon is one of the standard stars; and when this star passes the meridian in June, soon after mid-day, we may suppose FAHRENHEIT's thermometer at 70°; and when this star passes the meridian in December, near midnight, the thermometer may be at 30°. The mean refraction of this star at Greenwich, is $58''$ nearly. Therefore the refraction computed by the French table may, in summer, exceed that computed by BRADLEY's table by $0,''58$, and the contrary may

take place in winter. Hence an uncertainty in the index error, which as deduced from Procyon, might occasion a difference in the zenith distance of (ex. gr.) α lyræ in summer and winter, $= 1'',16$, bearing a considerable proportion to the supposed discordance in summer and winter. This is an extreme case: the index correction, as deduced from other stars, would not be so much affected. The polar star below the pole, is likely to be often used, and might occasion an uncertainty of about $1''$ under similar circumstances.

When I call this uncertainty, I suppose the matter is entirely doubtful as to the preference to be given to either of the formulæ.

Bradley's law of change from temperature was deduced from his astronomical observations, but other astronomical observations do not contradict the law of the French formula; which has also been confirmed by physical experiments, and seems more to be depended on.

It appears then that an incorrect law may materially affect the index error, and occasion incorrect results.

It therefore seems of the first importance with respect to a mural circle, to ascertain with exactness the law of variation of refraction from change of temperature; otherwise errors will be mixed up in all the conclusions.

It will not be possible to deduce easily, by the results from Bradley's refractions, the results that the French refractions would give.

For this purpose it will be necessary not only to know the mean temperature at the observations of the star, whose north polar distance is required, but also the temperature at the observations of stars by which the index error is com-

puted, which in fact is much the same as to recompute the index error,

I know not how far this may have been a source of inaccuracy in the north polar distances (p. 388, Phil. Trans. 1815), from the French refractions.

They seem to have been merely deduced from the column of N. P. D. by BRADLEY's refraction, and the mean heights of the barometer and thermometer, as given in page 386.

3. As to p, or the effect of parallax, we are not certain that many of the standard stars may not have a parallax in declination, amounting to a fraction of a second. This therefore so far will render the index error uncertain.

4. As to a, or the effect in declination of the aberration of light.

The maximum of aberration, pretty generally adopted of late years by astronomers, is 20″,25. The researches of the Chevalier DELAMBRE have principally led to this. The maximum formerly used was 20″. The former is probably more exact, but by no means certainly so. It is even possible that the maximum of aberration may be so low as 20″, or 19″,8. The strongest argument for $21\frac{1}{4}$″ is derived from the researches of M. DELAMBRE, respecting the reflected light from Jupiter's Satellites;* which certainly cannot be considered conclusive as to the direct light of the stars.

It seems reasonable to conclude, from an examination of Dr. BRADLEY's paper on aberration, that this matter requires farther examination, and that there is an uncertainty amounting to a quarter of a second.† If so, the index error com-

* DELAMBRE, Astron. Tom. 3, p. 105

† See note (A) at the end of this paper.

puted by the pole star may be uncertain o″,2 or o″,3, in July, and the same in an opposite direction in January. Add to this, it has not been usual for astronomers to consider the variable velocity of the earth in its orbit. The effect of this in N. P. D. as to stars, the $\mathcal{R}^s$. of which are nearly 3^s or 9^s is always insensible, but not as to stars, the $\mathcal{R}^s$. of which are nearly 0^s and 6^s, and are also far from the ecliptic. This quantity is nearly

$$= \frac{ab.\ \text{in N. P. D.}}{60} \cos.\ (\odot\ \text{Long.} \sim 9^s\ 9^\circ.)$$

and therefore in the pole star may amount in July to o″,34. Hence, from these two causes, the uncertainty in the aberration of the pole star in declination in July may be = o″,6. The joint effect of these causes will be o in October and in January.

The index error computed by the pole star when below the pole in July, will be opposite to the above, and thus the index errors so computed at the same time may differ 1″,2.

5. As to *n*. or the nutation, according to some astronomers, the nutation in declination,

$$= 7''{,}85 \text{ Sin. } (\mathcal{R} - \Omega) + 1''{,}15 \text{ Sin. } (\mathcal{R} + \Omega) \quad - \quad - \quad (1)$$

according to others,

$$8''{,}42 \text{ Sin. } (\mathcal{R} - \Omega) + 1''{,}23 \text{ Sin. } (\mathcal{R} + \Omega) \quad - \quad - \quad - \quad (2)$$

M. Laplace * made it (as will appear by the proper reductions) so great as

$$8''{,}76 \text{ Sin. } (\mathcal{R} - \Omega) + 1''{,}35 \text{ Sin. } (\mathcal{R} + \Omega \quad - \quad - \quad (3)$$

(1) is the nutation according to Lambert's tables. The maximum of formula (3) was deduced from the mass of the moon, $= \frac{1}{58{,}6}$, (the earth being unity), as determined by the

* Mec. Cel. Tom. 2, p. 350.

tides at the port of Brest. LAPLACE* afterwards modified this quantity, in consequence of the determination of DELAMBRE, as to the lunar equation of the solar tables; of the determination of Dr. MASKELYNE, as to the nutation itself from BRADLEY's observations; and of the determination of M. BURG, as to the parallax of the moon. These three determinations agree in deducing nearly the same mass of the moon, and induced LAPLACE to adopt $\frac{1}{68,5}$, and then the nutation will be nearly that in formula (2), which is nearly the same as that of DELAMBRE.†

LAPLACE has farther considered this subject,‡ and finds, according to a high degree of probability, that the maximum is between 9″,31, and 9,″94.

From the above there is evidently room for some uncertainty, which uncertainty may be doubled, by taking two stars differing 180° in right ascension.

M. DELAMBRE, although he thinks the maximum in aberration is settled, supposes the mass of the moon still subject to some uncertainty.§

6. As to *s*, or the semiannual equation as it is called. This = 0″,48 Sin. (2 ☉ — ☊). Here, on account of the smallness of the quantity of this equation, there is no room for any material uncertainty.

There is also another equation omitted by astronomers, viz. 0″,04 Sin. (2 ☽ — ☊). This cannot occasion a greater difference than 0″,08, and therefore scarcely need be noticed even among the minute objects of this enquiry.

* Mec. Cel. Tom. 3, p. 159.
† Astr. Tom. 3, p. 156.
‡ Conn. des Temps. 1818, p. 361
§ Conn. des Temps. 1810, p. 462.

7. As to *v*, or the variation in declination. This consists of two parts, one the effect of the precession of the equinoxes, and the other of the proper motion of the star. The former seems determined with sufficient accuracy. Also as far as regards the stars of the standard catalogue of Mr. POND, the latter seems pretty well agreed on among astronomers. But here arises a question of some importance: is the proper motion of each star uniform? It is assumed to be so in computing it by two results separated by a long interval. A series of results sufficiently accurate, and separated by intervals sufficiently long, have not yet been obtained to ascertain this important point.

A star of the 6th magnitude, 1 Leporis, seems to furnish an instance of a variable proper motion, by a comparison of the observations of BRADLEY and M. PIAZZI. There is nothing against a variable proper motion in our theories of the nature and motions of the fixed stars. Hence, another source of uncertainty in computing the index error.

8. Lastly, as to *c*, or the mean polar distance in the standard catalogue. This is subject to two uncertainties. The original error in the catalogue, and an uncertainty in the annual variation, as mentioned in the last article.

Notwithstanding all the care that has been used by Mr. POND in perfecting his standard catalogue, it may contain small inaccuracies, as will easily be apprehended from the observations in the preceding articles.

The uncertainties to which the index error is liable from the above causes, are independent of those to which the observation to which it is applied, is also subject. It may be

said, that these uncertainties tend to correct each other, and that the uncertainty remaining, after taking a mean of results from several stars, will be too small to be regarded. This indeed may be said as to the index error when applied to observations of the sun, moon, or planets; but not, I think, when it is applied to investigations relative to the parallax of the fixed stars, annual variation of north polar distance, exact determination of the quantity of aberration and nutation, and these, it will be allowed, are objects of great importance in the present improved state of astronomy.

Indeed, with respect to the parallax of the fixed stars, several of these objections may be obviated by a proper selection of the standard stars. Thus the uncertainties of refraction may be avoided by using only stars near the zenith. The objection in the 8th article may be partly obviated by using the same stars for ascertaining the index error at the two periods of greatest and least parallax, and so of other uncertainties. No error as to parallax arises from neglecting the unequal motion of the earth in its orbit, as far as regards the index error computed by the same stars at the two periods. But this selection of stars will be limiting the use of the instrument, and the advantage of a mean of a number of observations lost; and in fact, with respect to the index errors used in determining the N. P. D. of α Lyræ, α Aquilæ, and α Cygni, as given by Mr. POND, Phil. Trans, 1817, no particular selection of stars, with a view to these points, seems to have been made.

It may also perhaps be suggested, that the mural circle may be used without applying index error, as was done with

respect to the observations given in the appendix of Mr. POND's first paper. But the knowledge of the stability of the index error during six or eight months, depends on the reductions by the standard stars; and therefore the above sources of uncertainty remain. Mr. POND remarks, that between July and March the index error may have oscillated a small fraction of a second on each side the mean, and not more; so that I think no important conclusion can be deduced from the results in that appendix.

The differences between the exterior and interior temperature may have tended to exaggerate the discordance between the summer and winter observations made at Greenwich; but it may appear that sufficient observations have not been made to ascertain this point, when we consider the many other sources of uncertainty. As far as I have examined into this matter, with respect to my own observations, I cannot suppose any of my discordances materially affected by the difference of exterior and interior temperature. The room containing the circle at Greenwich is much smaller, and less lofty than the room of this Observatory, which contains both the circle and transit instrument.

I hope I have so expressed myself, that I shall be understood to mean, that I consider the results of observations hitherto made by the Greenwich circle inconclusive as to the existence or non-existence of parallax, merely from the uncertainty of the elements used in the reductions, not from any errors of the observations, or from any defects in the construction of the instrument.

I more particularly offer to the consideration of astronomers the preceding remarks, as in the present state of

astronomy, the relative fitness of instruments for ascertaining with precision the smaller motions, whether real or apparent, of the fixed stars, is an object of importance.

In instruments similar to that belonging to the Observatory of Trinity College, Dublin, the index error is found by reversing the instrument, the position of the vertical axis being ascertained by a plumb line. Thus the determination of the index error is not materially affected by any of the uncertainties above referred to. Therefore, by its principle, this instrument should appear particularly adapted for enquiries relative to the annual parallax, annual variation, &c. &c.

From the fixed telescopes we are probably to look for the final decision of the question of parallax. At first sight these seem to offer a very simple and certain criterion. However, a little consideration will point out probable sources of difficulty. Suppose the star under examination be compared with a star opposite in $\mathfrak{R}$, or with one as nearly so as can be conveniently had. Besides the uncertainty respecting the annual variation, even the uncertainty in the quantity of aberration may tend in some degree to conceal the parallax, unless the minimum of aberration in declination of each star be at the same time, and the observations are made pretty equally on both sides of this time. The star β Aurigæ has been judiciously chosen by Mr. POND to compare with α Cygni. A more proper star could not have been chosen; yet here the effect of an uncertainty in the maximum of aberration, amounting only to $\frac{1}{4}$ of a second, will have a sensible effect.

If we suppose the maximum only 20″, as I believe the maximum used by Mr. POND is 20″$\frac{1}{4}$, his winter distance for

the observations given would be increased 0″,2, and his summer distance decreased by about the same quantity; which would make his results differ in the same direction as they should do by the effect of parallax. I do not intend by this that any argument in favour of parallax can be deduced from his results, but only to show the effect of small uncertainties.

There may be uncertainty as to the stability of the instrument during the interval which elapses between the successive observations of α Cygni and β Aurigæ, which is sometimes necessarily of several days.

This is the point before alluded to; and there appears, on examining Mr. Pond's results as to α Cygni and β Aurigæ, indications of such an instability, and that to an amount that may do away the conclusion he has drawn from these observations.

The seconds of the micrometer for the same star should be the same in summer and winter, after the usual reductions, supposing no uncertainties in the elements of these reductions, supposing no parallax, and supposing no derangement in the instrument. Now, referring to Mr. Pond's paper, the seconds for α Cygni are decreased by about $\frac{1}{3}''$ in summer, and those of β Aurigæ increased by nearly the same quantity. This may be concluded to arise from a derangement in the instrument by the change of temperature, as Mr. Pond has mentioned no other cause. The effect of an increase of temperature, therefore, appears to be to decrease the seconds of α Cygni, and to increase those of β Aurigæ. Applying this to observations of the same day in winter, α Cygni passes the meridian near noon, and β Aurigæ near midnight, or at least

late in the evening. An increase of temperature, therefore, relative to α Cygni takes place, and the seconds in α Cygni become less than they would have been had the temperature remained the same as in the night. The sum of the seconds of α Cygni and β Aurigæ is diminished by this cause, and it would be increased by the effect of parallax. Hence this cause tends to conceal the effect of parallax in winter. In summer the passages of α Cygni and β Aurigæ are reversed as to noon, and the sum of the quantities increased by temperature and decreased by parallax.

This explanation, if justly founded, will have a tendency to diminish the value of stars nearly opposite in $\mathfrak{R}$, which Mr. POND so judiciously selected, and by which he avoided any uncertainty from differences of parallax. As to δ Cygni, the winter observations, Mr. POND remarks, are far from satisfactory; and they seem too few and too discordant to decide any thing, even supposing we were certain of the annual variation of δ Cygni, and that it had no visible parallax.

I shall now proceed to state briefly the results of my observations up to the present time, which appear to point out parallax as to α Cygni, α Aquilæ, and α Lyræ; also the results of observations of γ Draconis.

α Cygni

The winter observations of this star cannot be materially affected by any uncertainty in the maximum of aberration, being made nearly equally on both sides of the time when parallax is greatest, and aberration $= 0$. But the summer observations being generally made after the time when aberration in declination $= 0$, the effect of a less maximum of

aberration is to increase parallax. I have therefore used for my recent observations 20″4, and corrected my former ones, which were computed with 20″ max. of aberration; thus using the most unfavourable quantity.

Summer Z. dist. Jan. 1, 1815.

	N°. Ob.	° ′ ″
1811 & 1812	23	8 45 45,71 + ,74 p
1814	10	45,97 + ,72 p
1815	20	46,12 + ,48 p
1817	14	45,22 + ,68 p
	67	

Winter Z. dist. Jan. 1, 1815.

	N°. Ob.	° ′ ″
1810-13	24	8 45 47,12 — ,76 p
1814-15	12	46,79 — ,80 p
1816-17	16	46,57 — ,76 p
	52	

The correct means of the preceding results being taken by attributing to each a weight proportional to the number of observations, we obtain

$8° 45' 46'',86 - ,77\ p = 8° 45' 45'',77 + ,63\ p$

or $p = \frac{1,09}{1,40} = 0'',78$

or $2\ p = 1'',56$, the angle subtended by the diameter of the earth's orbit at the star.

α Aquilæ.

The stars β and γ Aquilæ pass the meridian within a few minutes of the passage of α Aquilæ; and as they are much inferior in brightness to that star, and differ less than 3 degrees in declination from it, I considered that if I could observe the three stars on the same day, the comparisons of the observations in winter and summer would furnish much information relative to the parallax of α Aquilæ.

As the stars pass so nearly together, there was not sufficient time to read off the three microscopes for each observation; I therefore, for some time, read off only the bottom microscope for γ, to be compared with the reading of the bottom microscope for α, and the three microscopes for α, giving up the observation of β. Afterwards, I only read off the bottom microscope for α, and thus was enabled to observe β. Unfortunately from the few observations to be obtained in October and November, when the sun approaches these stars, I have not succeeded hitherto in obtaining a sufficient number of observations; but my summer observations appear very satisfactory, in agreeing with the result from the former observations of these stars, which were made in the autumn of 1813, and with Mr. POND's north polar distances; whereas the summer zenith distance of α Aquilæ has been uniformly less than the winter zenith distance of that star. So that, as far as I have gone with this kind of trial, the results have been very strong in favour of the parallax of α Aquilæ. As in my recent observations of this star, only the bottom microscope has been used, I have deduced results from all my former observations of α Aquilæ from the bottom microscope only.

The conclusion as to the parallax of this star does not differ materially from my former one, where the three microscopes were used.

Summer Zenith dist. Jan. 1, 1817.

	N°. Ob.	by bottom microscope.
		° ′ ″
1808-1812	38	44 59 36,38 + ,38 p
1814	21	36,92 + ,21 p
1815-1816	22	36,10 + ,30 p
1817	25	36,54 + ,26 p
	106	

Winter Zenith dist. Jan. 1, 1817.

	N°. Ob.	by bottom microscope.
		° ′ ″
1808-1812	38	44 59 38,51 — ,40 p
1813-1814	24	39,20 — ,47 p
1816-1817	20	38,36 — ,45 p
1817-1818	20	37,57 — ,44 p
	102	

The correct means give

$$44^\circ\ 59'\ 36'',47 + ,30\ p = 44^\circ\ 59'\ 38'',86 - ,44\ p$$

$$p = \frac{1,89}{,74} = 2'',53$$

or $2\ p = 5'',0$ by 208 observations.

α Lyræ.

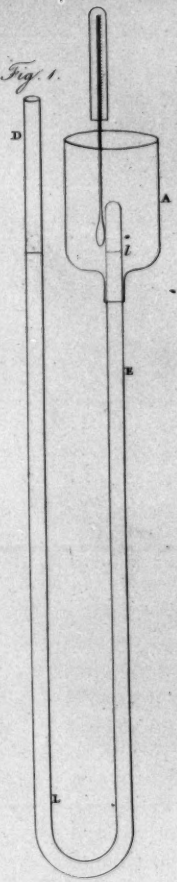
The following are the results of my observations of α Lyræ.

My former observations are here reduced to what they would be by the French refractions, and the other observations have been reduced, taking the maximum of aberration $= 20''.1$. Both circumstances tend to diminish in a small degree the parallax; but the result from all my observations gives the double parallax above $\frac{1}{2}$ a second less than I should have expected from my former observations. Whether the discordance I had found was to be attributed to parallax, or any other cause, I had expected the new results would not materially differ from my former conclusions. Although it has happened otherwise, yet an examination of the different results will, I conceive, be found not to contradict my former remarks respecting the accuracy to be attained by my instrument.

Summer Zenith dist. Jan. 1, 1811.

	N°. Ob.	° ′ ″	
1808-1813	65	14 46 19,85	+ ,78 p
1814	20	19,87	+ ,78 p
1815	20	19,86	+ ,74 p
1816	11	20,46	+ ,77 p
1817	12	19,62	+ ,62 p
	128		

Fig. 1.



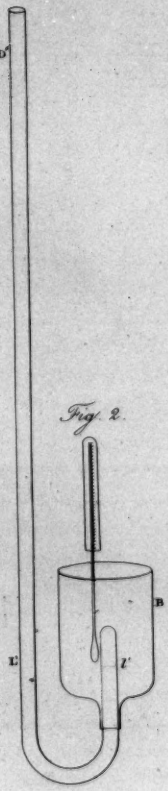


Fig. 2.

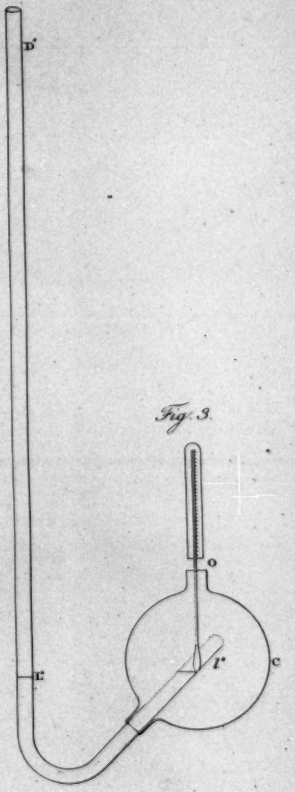


Fig. 3.

Winter Zenith dist. Jan. 1, 1811.

	N°. Ob.	° ′ ″	
1808-1813	61	14 46 20,96	— ,79 p
1814-1815	20	21,00	— ,78 p
1815-1816	14	19,47	— ,68 p
1816-1817	15	20,88	— ,76 p
1817-1818	24	20,06	— ,76 p
	134		

The correct means give

$19'',63 + ,76\ p = 20'',64 - ,77\ p$

or $p = 0'',66$

or $2\ p = 1'',32$, the result of 262 observations of

α Lyræ.

γ Draconis.

Of this star, the mean of 53 observations in ° ′ ″

winter gives mean Z. D. Jan. 1. 1814. = 1 52 17,55

59 observations in summer give = 1 52 17,92

This result is in a direction contrary to parallax, and therefore had I compared the differences of zenith distances of this star and α Lyræ, in summer and winter, the result would have given me a greater parallax for α Lyræ.

This conclusion is quite opposite to that of Mr. Pond, and seems to me a point of much difficulty to be explained. However, from the mean of my late results as to α Lyræ, I am inclined now, to consider my former argument deduced from γ Draconis of less weight than I had attributed to it, not thinking the observations of γ Draconis sufficiently numerous.

I have thus stated the results of my observations, and the conclusions that seem to follow as to the parallax of the respective stars. The many causes that may lead, if not to actual error, at least to a high degree of uncertainty, induced me in the paper alluded to, to speak with hesitation as to my explanation. The observations of Mr. POND, as far as they go, seem to invalidate that explanation, particularly as to α Cygni and α Lyræ.

It is by observation alone that the decision can be made. No conjecture as to the relative distances of the stars can be of any material weight. The conjecture, in itself probable, that the brightest stars are nearest to us, seems opposed by another conjecture, also by itself probable, that those stars are nearest which have the greatest proper motion.

Some of the brightest fixed stars have scarcely any sensible proper motions, while those of some much smaller are very perceptible. The two stars, 61 Cygni, have each an annual proper motion of about 5″,3 in right ascension, and of 3″ in declination. These stars are of about the 6th magnitude, and one a little brighter than the other.

This great proper motion seemed to render it probable, that these stars are sufficiently near to us, to have a visible parallax. I accordingly made observations on one of them, but found nothing satisfactory.

Also 40 Eridani, which is of the 5th magnitude, has so great a proper motion, that we might conjecture it to be nearer to us than many of the brighter stars.

The uncertainty, therefore, respecting the relative distances, as deduced from their degrees of brightness, weakens conclusions against parallax drawn from differences of north

polar distances of stars having nearly the same right ascension, and north polar distance.

It would be an interesting circumstance, could the existence of visible parallax in any one star be ascertained, and placed beyond doubt, by the joint results of two separate instruments. The comparison of my summer and winter observations of α Aquilæ indicating so great a parallax, induces me to expect that as to this star it may yet be accomplished.

Mr. Pond suggests that the effects of refraction may occasion some uncertainty as to this star. This can only arise from irregularity of refraction; and it seems scarcely possible that the mean of 100 observations can be sensibly affected thereby. My refractions have been computed from the internal thermometer placed on the instrument: had they been computed by the external thermometer, the difference between the summer and winter zenith distances of α Aquilæ would have been lessened about 0″,3. As α Aquilæ passes the meridian near noon in winter, there is seldom much difference then between the external and internal thermometer here.

If the discordance which I have found between the summer and winter zenith distances had arisen from the different temperatures at the two seasons, it might have been expected that Aldebaran, Capella, α Orionis and Procyon would have been much more affected by this cause; as in winter they pass the meridian at night, and in summer in the day time; and therefore as to these the observations are made in the extremes of temperature.

To many, the time and labour spent in this minute enquiry, may appear wasted. Some however will justly appreciate

the exertions that have been made here and at Greenwich. Several attempts to observe the parallax of the fixed stars have failed since the time of Dr. HOOKE, and Mr. FLAMSTEAD,* and if this should end like the rest, it will be some satisfaction to have ascertained, beyond doubt, certain limits; and also, probably to have occasioned these limits to be still farther circumscribed by the observations of Mr. POND, in the event of his not confirming my conclusions.

* See note (B).

Observatory of Trinity College,
Dublin, Feb. 20, 1818.

Note (A).

Upon examination of Dr. BRADLEY's* account of the aberration, it will appear, I think, that the maximum of aberration deduced therefrom, cannot be depended on to $\frac{1}{4}$ of a second. Dr. BRADLEY afterwards mentions, in his paper on the nutation, that he had revised his computation, and states 20″ as the result nearest the truth. The result from the eclipses of Jupiter's satellites, as deduced by M. DELAMBRE, is $20\frac{1}{4}$. The limit of the probable error of this latter determination is not easily known; but it appears to me that we ought to adopt the result of Dr. BRADLEY's revision, rather than any conclusion *we* can deduce from the data in his first paper. We have not the original observations to refer to; and it is to be remarked, that he puts down all the maxima of changes of

* Phil. Trans. xxxv, 637, or Old Abridg. vi, 149.

declination (D—D′) in seconds, without fractional parts, and thence deduces for each star the maximum of aberration.

	D—D′	2 *a*	2 *a* †
γ Draconis	39″	40,4	40,3
β Draconis	39	40,2	40,2
η Ursæ Maj^{r}.	36	40,4	40,4
α Cassiopeæ	34	40,8	41,1
τ Persei	25	41,0	41,4
α Persei	23	40,2	40,2
35 Camel.	19	40,2	40,2
Capella	16	40,0	39,7
	Mean	40,40	40,44

I re-computed the maximum of aberr. from column D—D′, and find the results as in column 2 *a* †, which differ a little from BRADLEY's results in column 2 *a*, but differ considerably from M. ZACH's results (Conn. des Temps. 1810, p. 459) as to τ Persei and 35 Camel. However, M. ZACH seems not to have attended to Dr. BRADLEY's remark respecting τ Persei, and therefore, as to this star, his result is erroneous; and his different result from 35 Camel. must have been an error of computation.

It is evident both from the consistent results, and Dr. BRADLEY's remarks, respecting the annual variation, which he deduced from observation, that his observations from whence the above results have been deduced, must have been extremely accurate. The changes of nutation (evident by his observations, but at that time unknown to him) are included in the annual variation; and hence no source of error on this

account, except from error of observation: therefore we may conclude that had Dr. BRADLEY used the fractional parts of the seconds, the maximum would have been very accurate. We cannot now estimate the effect of this omission; we can only see that it is probable it has had a sensible effect on his conclusion; and we may suppose this to have been corrected by his subsequent revision. His words in the paper * on nutation are, "I have again examined my observations that were most proper to determine the transverse axis of the ellipse, which each star seems to describe, and have found it to be nearest to 40"; which number I therefore make use of in the following computations." He had at first concluded it to be $20''\frac{1}{4}$. On the whole then, it seems that 20" is the result deduced from the direct light of the fixed stars, and $20''\frac{1}{4}$ from the solar light reflected from Jupiter's satellites. It is highly probable that future observation will find these quantities exactly equal. At present there exists an uncertainty.

Note (B.)

The results of the attempts of HOOKE and FLAMSTEAD are remarkable; the former reasoned justly on inaccurate observations, and the latter wrong on exact ones; and both imagined they had discovered a parallax. HOOKE, who erected at Chelsea a fixed telescope, 36 feet long, for observing γ Draconis, found a change of place agreeing with a considerable parallax. The great mechanical skill of Dr. HOOKE, the length of his telescope, and the precautions he took, seemed to leave no doubt.

Dr. BRADLEY, in his paper on the aberration, expresses great

* Phil. Trans. xlv, 1; or Old Abridg. x, 32.

surprise at the erroneous results of HOOKE's observations; which he, Dr. BRADLEY, had considered as exact, till they were contradicted by Mr. MOLYNEAUX's observations, and by his own. He says, "I cannot well conceive that an instrument of the length of 36 feet, constructed in the manner he describes his, could have been liable to an error of 30″ (which was doubtless the case), if rectified with so much care as he represents."

It may be remarked here, that the results of the observations of α Cygni with the fixed telescope of Mr. POND, as given in the last volume of the Transactions, would in themselves appear to indicate a considerable parallax in that star, and thus produce an error similar to that of HOOKE. But Mr. POND guarded against such a source of error, by using two stars; and therefore no derangement of the instrument could affect his results, except as far as it might take place between two succeeding observations.

FLAMSTEAD's instrument, which he has described in his letter to Dr. WALLIS,* was a mural arch of 140°, by which he could observe all stars visible in his hemisphere; and observe below the pole all circumpolar stars that were not above $11°\frac{1}{2}$ from the pole. He deduced the index error of his instrument by observations of the pole star corrected for refraction; not at first considering any correction for parallax as necessary: but he soon found a correction necessary, which he attributed to the effect of parallax. In this he was singularly mistaken. The result of his observations was, that the diameter of the circle described by the pole star about the pole, was 1′ 20″, or 1′ 30″ greater in summer than in winter.

* Wallis Oper. tom. iii. p. 701.

This we now know to be the effect of the aberration of light. Thus FLAMSTEAD's observations give the maximum of aberration 20″, or 22″$\frac{1}{2}$. The near agreement of this with BRADLEY's result is much greater than could have been expected from FLAMSTEAD's instrument; but the remarkable circumstance is, that FLAMSTEAD should have been so much mistaken in his mathematical application; and that WALLIS, who interested himself much in the question of parallax, did not point out his mistake: it can only be attributed to the great age of WALLIS, who was then in his 83rd year.

XV. *On the urinary organs and secretions of some of the amphibia.* By John Davy, *M. D. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.*

Read April 2, 1818.

Colombo, March 25th, 1817.

THE urinary organs of the amphibia have been imperfectly described by authors; but I am not aware that any account has hitherto been published of the urinary secretion of any of this class of animals.

Since I have been in Ceylon, both subjects have excited my attention, and on both I have had favourable opportunities of gratifying my curiosity. It may not be uninteresting to the Society, to know the results of my observations. I shall briefly state them, confined as they are at present to a few animals of four natural families.

1. *Of the urinary organs, and urine of serpents.*

The kidneys of the different kinds of serpents I have examined, resemble each other generally; though in each kind, there are minute and trifling differences. In every instance, the kidneys are very large, nearly equal in size to the liver; they are long and narrow, and very lobulated; like some of the mammalia with conglomerate kidneys, they are destitute of a pelvis; each lobule sends a small duct to the ureter, which leaves the kidney in two branches. The ureters in general terminate in a single papilla. The papilla is situated in the

cloaca between the mouths of the oviducts; it is a little elevated above the surface, and its point is directed towards a receptacle into which the urine enters. The receptacle is a continuation of the intestine, yet it may be considered distinct both from the rectum and cloaca, with both of which it communicates only by means of sphincter orifices. This conformation of parts may be seen to advantage in large species of snakes. I first observed it in the rock-snake and the rat-snake, two species of coluber, frequently found from eight to ten feet long.

The urinary ducts of serpents are frequently of an opaque white colour, from a white matter which they contain, which is visible through their transparent coats, and which may be expressed and collected from the papilla in small quantities for examination. More or less of a similar white matter is almost constantly found in the receptacle; generally it is found in soft lumps, rarely in hard masses. In the receptacle, I have always observed it pure and entirely free from fæcal matter. This solid urine, for such it is in reality, gradually accumulates in the receptacle, till it forms the masses just described. It is a long time thus collecting, from three weeks to a month or six weeks. When the bulk of the masses is so considerable as to distend the part, they are expelled by an unusual exertion of the animal, most commonly in the act of devouring its food, which it takes periodically, at intervals of from three to six weeks. The urine is voided occasionally, accompanied by, but never mixed with, fæces. When expelled, it is commonly in a soft state, of a butyraceous consistence, which it loses from exposure to the air, and becomes hard and like chalk in appearance. This change is produced, I believe, merely by the evaporation of moisture. The quantity of solid urine secreted

by snakes is very great, more even than might be expected from the size of their kidneys; it is not unusual to see masses weighing three or four ounces, voided by large snakes.

The chemical nature of this urine was such as I expected to find it; I say expected, because before I left England, I was told by Dr. Prout, that he had examined the excrement of a serpent in London, and had ascertained that it was nearly pure uric acid; such have I found it here in every instance, in at least eight that I have tried it; and the properties of that fresh from the ureter, were precisely the same as of that contained in the receptacle, or of that voided. Before the blow-pipe, it emitted strong ammoniacal fumes, consumed without flame, and afforded only a very minute quantity of ash, consisting chiefly of phosphate of lime, and a fixed alkaline phosphate, and a little carbonate of lime; in muriatic acid it was insoluble; in warm dilute nitric acid it was soluble with effervescence; and the solution evaporated, afforded the pink residue almost peculiar to uric acid; in an alkaline ley it was soluble, and the solution was precipitated by muriatic acid. These properties sufficiently prove, that the nature of the urine of snakes is as above stated. Besides uric acid, I have not been able to detect any other ingredient, nor do I believe that the urine contains any other, with the exception of a little dilute mucus, with which it is mixed and lubricated.

2. *Of the urinary organs, and urine of lizards.*

I have examined the urinary organs of four different species of lizard, the gecko iguana, a large species resembling the iguana, called by the natives kobbera-guion,* and the

* For an account of this animal, see Knox's History of Ceylon.

alligator. The shape of the kidney varies in different instances; to each ureter there is a papilla, and the papillæ are situated in the receptacle itself; and in no other respect have I been able to discover between the urinary organs of these lizards and of snakes, any material difference. Neither does the urinary secretion of these four species, and of many other species that I have examined, differ from that of snakes in its essential nature; in every instance I have found it nearly pure uric acid. The uric acid of the alligator contains a large proportion of carbonate and phosphate of lime. Two specimens of this urine, from different alligators, agreed in this circumstance; they differed however in one having no odour, and the other a strong one of musk; the former was from a very young, the other was from an older animal.

3. *Of the urinary organs and urine of the turtle and tortoise.*

The kidneys of the testudo mydas, and geometrica, the only species I have hitherto examined, resemble those of the preceding animals in their lobulated structure. The proportional size of the kidney of snakes is greatest; that of lizards next; and that of the animals we are now considering, least.

In the bladder, both of the turtle and tortoise, I have found flakes of pure uric acid, but in no great abundance: it was in a transparent watery fluid, containing a little mucus and common salt, but no urea or any other substance that I could detect in the small quantity on which I operated.

It is curious to observe the links by which animals, in appearance totally dissimilar, are connected together. That there should be so close an analogy between the urinary organs and secretion of the serpent, lizard, and testudo, is

not surprising, their organic structure, and their habits and economy being so similar; but that an analogy should exist between animals so very different in general appearance as birds and amphibia, is not a little singular, yet it is true: the urinary organs of one class, as well as the lungs, primæ viæ and genital organs, resemble those of the other, and both are peculiar in secreting uric acid; those living entirely on animal food secreting it pure.

XVI. *On a mal-conformation of the uterine system in women; and on some physiological conclusions to be derived from it. In a letter to Sir* Everard Home, *Bart. V. P. R. S. from* A. B. Granville, *M. D. F. R. S. F. L. S. Physician in ordinary to H. R. H. the Duke of Clarence; Member of the Royal College of Physicians, and Physician-Accoucheur to the Westminster General Dispensary.*

Read April 16, 1818.

Dear Sir,

When I proceeded, two years ago, to Paris for the purpose of studying still farther, and in a more advantageous manner, the particular branch of the medical profession I have embraced, you did me the honour to request that I would communicate to you any fact, which might occur to me during my stay in that capital, calculated to elucidate the process of generation. The good fortune I enjoyed in being admitted into the *Maternité*, or Great Lying-in Hospital, than which no other similar public institution in Europe, except perhaps that at Vienna, is better calculated to advance the study of the obstetric art; and the frequent anatomical investigations at which I assisted, through the kindness of Mons. Chaussier, one of the most distinguished professors of the Faculty of Medicine, have, I am willing to believe, placed it within my power to satisfy you on the subject of your request.

Of the many facts, however, which the numerous opportunities I possessed have brought before me, connected with the physiology of generation, none, in my opinion, deserves more

to be mentioned than the one which it is the purport of the present letter to describe; and when I reflect on the many important researches you have made on this interesting subject, I feel confident, that you will find the contents of the following pages worthy of your attention; since the case they record appears to me to stand single in the annals of descriptive anatomy. It is for this reason, therefore, that I shall confine myself to its description alone, leaving any other information I may possess on the subject, as matter for future correspondence.

Early in June 1817, I was summoned to attend the opening of the body of a woman aged 40, who died at *La Maternité*, six or seven days after delivery, of what had long been suspected to be a disease of the heart, or of some of the larger vessels. This conjecture was not ill founded; for on examining the contents of the thorax, an aneurism of the aorta was discovered, together with a considerable enlargement of the heart. As a subject for secondary consideration, it may not be improper to mention, also, that the bronchiæ were found lined by a beautifully formed membrane, entirely detached from their inner surfaces, and admitting of being extracted without deranging its tubiform structure; yet, the patient had never complained of any disorder of the respiratory organs.

Our attention, however, during dissection, was soon engaged by a still more curious anatomical arrangement, which presented itself to our view on the examination of the abdominal viscera. The womb, half concealed by the intestines, and immersed in a considerable quantity of serous fluid, being four times at least the size of what it is in the unimpregnated state, was lying in its natural position. On clearing it from all the surrounding parts, it was found that this viscus had acquired

its full developement on the right side only, where it presented the usual pear-like convexity and undulation; while the left, exhibited a *direct straight line*, scarcely half an inch distant from the centre, although more than two inches could be measured from that same point to the outline of the right side. But this is not all: the spermatic and uterine vessels, the Fallopian tube, and the ovarium, with its surrounding peritonæal folds (which on the right side were placed as usual, and in their natural state, with the regular opening into the fundus uteri) were not to be found in the left. The rudiments, for they could be called by no other name, of those appendages of the uterine system on that side, were discovered lying in the inferior part of the cavity of the pelvis, loosely connected with the cervix of the uterus; the ligamentum rotundum being inserted into the superior and interior ridge of the os pubis of the same side. In dissecting this confused mass, we perceived what may be said to have been an ovarium, shrivelled, horny, and dry, and lost within the intricacies of the substance to which it was attached.

None of the other parts of generation presented any particular appearance worthy of remark.

I have endeavoured to pourtray the peculiar arrangement of the parts above described in the accompanying sketch, taken at the time. (Pl. XVII.)

This woman had been the mother of eleven children of both sexes, and had, as I have before observed, been delivered a few days previously of twins, a male and a female.

From this latter circumstance, in particular, we are enabled, for the first time, to form a definite opinion regarding the theory which has been advanced, at different times, respecting

the cause that has been supposed to influence the procreation of the two sexes: and to answer effectually, if an answer to such a proposition can be thought necessary, the arguments adduced by some systematic writers on the subject, but more particularly by Mons. MILLOT, in a recent work, "on the art of procreating the two sexes at pleasure;" in which he lays down distinct precepts for ensuring to parents either a male, or a female succession. Most of these systems, and that of the latter physiologist more especially, are founded on the ovarium and the other uterine appendages being always double; and it is curious to reflect, that however gratuitous such an idea might seem, no proof to the contrary has ever been brought forward before, to my knowledge.

The case I have now recorded, however, expunges of course for ever from our books of physiology such an hypothesis, and leaves us to discover a more plausible reason for this peculiar and very interesting feature in the process of generation.

It moreover puts to rest our doubts respecting a phenomenon of even greater importance, the existence of which, every physiologist, who has treated on the subject, with the exception of yourself, seems to have been disposed to deny. I allude to the fact of the ova of twins, and those of different sexes too, coming from the same ovarium; when, as in the present case, the fimbriated part of the Fallopian tube must necessarily remain attached for a considerable length of time to the ovarium, so as to allow both ova to pass into the uterus, without any union taking place between their membranes at any period of their progress through the tube, and afterwards coming in contact with a different part of the womb, to which they adhere. These circumstances necessarily suppose a dis-

tance between the ova, which distance cannot exist without two distinct momenta in the propulsion of the two ova from the ovarium into the oviduct.

What obtains in cases of twins, must also take place where three, four, or even six children have been brought forth at one birth; three ova being formed in each ovarium, so near to each other, that the three points at which they were expelled, must have all come within the long continued grasp of the fimbriated bodies. Indeed it is possible, that the separation of more than one ovum at a time, from one ovarium, or from both, takes place oftener than we are led to believe, from the mere calculation of the number of complex labours. For if the ova pass into the uterus even without copulation; it is no bad logic to suppose, that some will occasionally go through that viscus, which are never impregnated at all. This is indeed proved by a greater number of corpora lutea being found in the ovaria of some females, than corresponds to the number of young they have produced; a doctrine, which I first read in your valuable communication in the Philosophical Transactions, "*On the Passage of the Ovum in Women, &c.*" when you last year gave me the paper to carry to Monsieur CUVIER, and which I was happy to find afterwards immediately adopted by that eminent naturalist, in his course of lectures on generation.

These remarks lead to another, and the last consideration on the present subject; namely, the existence of separate placentæ in cases of twins, or even triplets; which unless viewed in its true light, will make us run the risk of rendering that obscure, which in the process of generation seems perhaps the most intelligible. Instances of such occurrences are very numerous; they have been often mentioned by

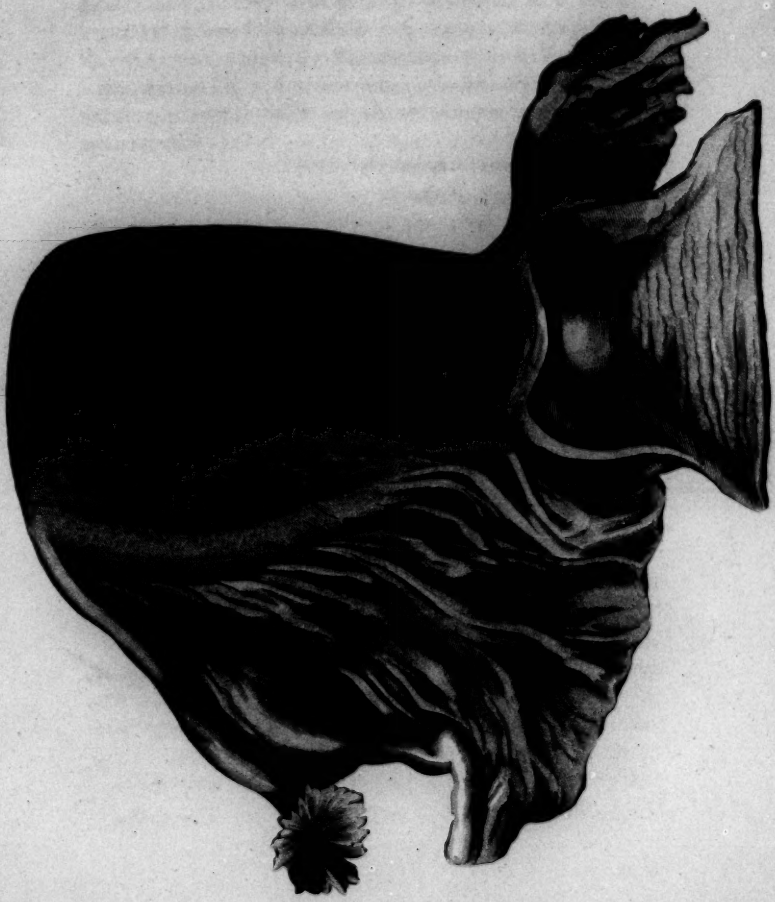
authors of veracity, and are recorded in the registers of the Great Lying-in Hospitals at Paris and Vienna. Within the last eighteen months, four cases have occurred within my own practice; and I have at this moment in my possession, the preparation of two placentæ, one of which was expelled several days before the membranes of the second were ruptured. In the case I have related, both the placentæ and membranes came away separately, and at different periods; proving still farther the soundness of the foregoing reasoning, giving a greater weight to the present observations, and explaining a number of cases in the ordinary process of generation, which have been brought forward as cases of superfetation.

In one of the volumes of the Transactions of the Royal College of Physicians, is a paper entitled "A case of superfetation," which merely goes to prove the occasional coexistence of separate ova in utero, and proves nothing farther.

The Lady, whose prolific disposition is much descanted upon in that paper, and with whom twin cases were a common occurrence, was delivered of a male child sometime in November, 1807, "*under circumstances very distressing to the parents, and on a bundle of straw;*" and again in February, 1808, (that is, scarcely three months afterwards) of another male infant, "*completely formed!*"—mark the expression, for it was not made use of in describing the first. The former died "*without any apparent cause,*" when nine days old; the other lived longer. Now, can we consider this otherwise than as a common case of twins, in

which one of the fœtuses came into the world at the sixth, and the other at the ninth month of pregnancy, owing to the ova being quite distinct and separate? Had this not been the case, the *distressing circumstances*, which brought on the premature contraction of the womb, so as to expel *part* of its contents in November, as in the simplest cases of premature labour, would have caused the expulsion of the whole, or in other words, of both ova, in that same month; and we should not have heard of the second *accouchement* in the following February; which led the mother of the Lady in question to bring the case forward as one of superfœtation, in opposition to what he has called "the scepticism of modern physiologists." Had it been proved that the child, of which the Lady in question was delivered, had *reached its full term* of utero-gestation in November, and that she had brought forth another child one, two, or three months afterwards, of equally full growth; then a case something like superfetation would have really occurred, and scepticism would have been staggered.

I have now under my eyes a recent preparation from Mr. CHAPMAN, at Windsor, destined for Mr. CLARKE's collection; but which through the kindness of Dr. BAILLIE I have been allowed to examine for my private information; where the *complete* ovum is seen, such as it was expelled at the seventh month of pregnancy; the Lady being safely delivered of another child alive, two months afterwards. Although the first fœtus was expelled at the seventh month, it was evidently of a growth of a shorter period, and had remained in the uterus dead for three months. When the case was read before the Medico Chirur-



gical Society, I ventured to give an explanation of it, similar in point to the one I have offered with regard to the Lady from Palermo, mentioned in the Transactions of the College; and I was happy to find that every one present coincided in my opinion; throwing quite out of the question every idea of superfetation.

I have the honour to be, Dear Sir,

yours truly,

A. B. GRANVILLE.

Saville Row.

XVII. *New experiments on some of the combinations of phosphorus.*

By Sir H. Davy, *LL.D. F.R.S. Vice Pres. R.I.*

Read April 9, 1818.

In a paper published in the Transactions of the Royal Society, for 1812, I have detailed a number of experiments on phosphorus, from which I deduced the composition of some of its compounds with oxygen, with hydrogen, and with chlorine. Since the appearance of this paper, various researches have been brought forward on the same subject, in which some results, differing very much from each other, and from mine, are stated. I ventured to conclude that the phosphoric acid contained double the quantity of oxygen to that in the phosphorous acid; and that phosphoric acid contained about $\frac{1}{5}$ of its weight of oxygen.

M. Berzelius considers the oxygen in phosphoric acid to be 128.17, and M. Dulong, 124.5, the phosphorus being 100. M. Dulong and M. Berzelius suppose the quantity of oxygen in phosphorous acid to be to that in phosphoric acid as 3 to 5.

The motive which immediately induced me to resume the enquiry respecting the phosphoric combinations, was M. Dulong's paper. This ingenious chemist has not only endeavoured to establish new proportions in the known compounds of phosphorus, but has likewise attempted to prove the existence of two new acids of phosphorus; and has denied several facts which I considered as sufficiently established.

The details which I have to lay before the Society in the

following pages, will serve to correct and fix, I hope, with tolerable accuracy, the proportional number or equivalent of phosphorus, and at the same time will show the truth of the general series of proportions that I assigned to its compounds. In a case where my conclusions differ so materially from those of M. BERZELIUS and DULONG, it may be supposed that I have not adopted them without considerable caution; and I have preferred my own results to theirs, only because they have been confirmed by minute and repeated experiments.

I was certain from various experiments, made both long ago and recently, and the results of which had been confirmed by Mr. BRANDE, that the proportion of oxygen, which M. DULONG assigns to phosphoric acid, is considerably smaller than that denoted by the combustion of small quantities of phosphorus in oxygen gas. I knew that minute portions of phosphuretted hydrogen were separated from phosphorus by voltaic electricity; and it occurred to me as possible, that water might be formed in the combustion of phosphorus, and separated from the phosphoric acid when it entered into saline and metallic combinations. To ascertain if this were the case, I passed phosphorus to saturation through red hot lime in a green glass tube connected with a mercurio-pneumatic apparatus: the combination took place with vivid ignition; but no elastic fluid was produced. A portion of the phosphuret of lime formed, was introduced into a tray of platinum, and heated in a glass retort filled with oxygen gas; the phosphuret of lime burnt brilliantly, and became partly converted into *phosphate of lime;* but on restoring the original temperature of the retort, there was no appearance of *vapour* or of *moisture*.

Having examined the phosphate of lime formed in this

operation, and satisfied myself that it was the same as that formed by other methods, it became evident that there were no sources of error in the experiments on the combustion of phosphorus in oxygen gas, arising from the formation or separation of water; and the only circumstance which could be urged against the accuracy of processes on this combustion, was the small quantity of materials* on which they had been made.

The vividness and rapidity of the combustion of phosphorus, renders it impossible to burn considerable quantities of phosphorus in the common way in glass vessels. Phosphuret of lime burns much more slowly and less intensely. I endeavoured to ascertain the quantity of oxygen absorbed by a given weight of phosphorus converted into phosphuret of lime; but the experiment did not succeed. Though the phosphuret of lime was in fine powder and distributed over a large surface, yet the phosphate of lime which formed and fused on the exterior, defended the interior of the phosphuret from the action of the oxygen, and prevented its combustion.

After various unsuccessful trials to convert considerable quantities of phosphorus into phosphoric acid by combinations containing oxygen, I at last thought of a very simple mode of burning phosphorus, which answered perfectly.

Phosphorus requires a considerable heat for its volatilization. By inclosing it in a small tube, so constructed that the phosphorus can burn in vapour only from the aperture of

* A source of error might be suspected in carbon combined with phosphorus; but I have been convinced by experiments made on the action of chlorine on the phosphorus I employed, that it contained no appreciable quantity of carbon. I suspect that what is often taken for carburet of phosphorus, is in reality a red oxide.

the tube, large quantities of it may be burnt by the heat of a spirit lamp in a retort filled with oxygen, and the absorption of oxygen and the quantity of phosphoric acid formed may be minutely ascertained.

The accompanying sketch (Pl. XVIII.) will give an idea of the apparatus. The neck of the little curved tube, or small distilling retort, after the phosphorus is introduced, is drawn out, and an aperture left of about $\frac{1}{10}$ of an inch; it should not be smaller, or it becomes choaked by the phosphoric acid formed. Regulating the heat by raising or lowering the spirit lamp, the combustion may be carried on slowly, or rapidly, at pleasure.

Operating in this way, I have often burnt from 5 to 10 grains of phosphorus without any accident, and ascertained exactly the quantity of oxygen absorbed: there is only one source of error—a quantity of phosphorus remains in the upper part of the tube, which cannot be burnt except by a greater heat than the retort will bear; and it is difficult to ascertain the precise weight of this, as the tube always unites with some phosphoric acid where it is red hot at its mouth; but this can be only a trifling source of error.

In these experiments, and in all the others detailed in this paper, I received much useful assistance from Mr. Faraday of the Royal Institution; and much of their value, if they shall be found to possess any, will be owing to his accuracy and steadiness of manipulation.

Experiment I.

Six grains of phosphorus. The small tube with the phosphorus weighed before the combustion 56.5 grains; after the

combustion 50.9; so that it had increased $\frac{4}{10}$; and this increase was in great measure from phosphorus that had escaped combustion; and when this was burnt out by a strong red heat, the increase of weight of the tube was under $\frac{1}{10}$; so that at least 5.9 of phosphorus had been converted into acid: 23.5 cubical inches of oxygen were absorbed: thermometer being at 46° FAHRENHEIT; barometer 29.6 inches.

EXPERIMENT II.

Ten grains of phosphorus. The glass tube containing the phosphorus weighed 103.1 grains; after the experiment 95.6; but much phosphorus remained unconsumed. After the tube had been heated to redness, it weighed 94 grains; so that at least 8.4 grains of phosphorus were consumed in the first process. The absorption of gas was 34 cubical inches. Barometer 29.8, thermometer 47°.

EXPERIMENT III.

Ten grains of phosphorus. By weighing the tube after the experiment, and then distilling and burning the residual phosphorus, it was found that 9.1 grains of phosphorus had been burnt, which had absorbed 33.25 cubical inches of oxygen. Barometer 29.7, thermometer 49° FAHRENHEIT.

I give these experiments as the most accurate I have made. The pressure and temperature vary so little, that the corrections for them are of no importance. Supposing that 100 cubical inches of oxygen (the barometer being between 29.8 and 29.6, and the thermometer between 46° and 49° FAHRENHEIT) weigh 33.9 grains, phosphoric acid will be composed, according to the first result, of 100 phosphorus to 135 oxygen;

according to the second, of 100 to 137.2; and according to the third, of 100 to 131.8: the mean will be 100 to 134.5.

The light of the phosphorus burning in vapour in these experiments was excessively bright; yet the top of the retort never became softened; and the phosphoric acid, which increased the weight of the tube, principally combined with the glass at the aperture where it was red hot. I cannot but consider this process of burning phosphorus in the gaseous state in a great excess of oxygen, as the most accurate mode that has yet been devised for ascertaining the composition of phosphoric acid. In this instance no phosphorous acid, as I ascertained by direct trials, is formed from the vapour; and no substances are concerned except those that actually combine. M. Dulong's method of ascertaining the composition of phosphoric acid, appears to me much too complicated to afford any results approaching to accuracy. He first combines copper wire with phosphorus, by passing phosphorus over it by means of a stream of hydrogen gas; he then dissolves his phosphuret of copper in nitric acid, and determines the quantity of phosphoric acid formed by precipitation: in all of which processes sources of error may exist.

M. Berzelius's methods of ascertaining the composition of phosphoric acid, that of reviving gold from its oxide by means of phosphorus, and that of determining the quantities of phosphate and muriate of silver formed from perphosphorane, or the perchloride of phosphorus, appear to me still more exceptionable; yet his results on the quantity of oxygen approach nearer to mine than those of M. Dulong.

The facts which I endeavoured to establish respecting chlorine, in a paper published in the Philosophical Transac-

tions for 1810, show that the proportional or equivalent volume in which chlorine combines, is to that in which oxygen combines, as 2 to 1; and it follows, that 10 grains of phosphorus in forming the white sublimate, or perchloride, ought to combine with between 76 and 80 cubical inches of chlorine.

In experiments that I formerly made on this subject, by admitting chlorine to phosphorus in exhausted vessels, and ascertaining the absorption by introducing solution of chlorine, I overrated the absorption. I did not at that time know, what I have since ascertained, that a solution of chlorine in water, *apparently* saturated with chlorine, by agitation with it in long narrow vessels, will still take up more, by exposure to a great surface of chlorine in larger vessels. Under all circumstances, it is difficult to gain very precise results in experiments on the action of phosphorus on chlorine. Mercury acts so rapidly upon chlorine, that it cannot be employed in experiments in which the absorption is to be determined. When common water is used, some of the gas is absorbed by the water, and, the sublimate being a very volatile substance, its vapour always increases the volume of the residual gas. Some aqueous vapour likewise, in experiments over water, enters with the gas, which forms a volatile hydrate, the effect of which is likewise to diminish the apparent absorption of chlorine.

I have always found the absorption greatest, when I have operated in small retorts, connected by small stop-cocks with the vessel containing the chlorine, over water. Making the proper corrections for the absorption by the water, the apparent absorption has been from 35 to 38 cubical inches for every 5 grains of phosphorus.

M. Dulong's two methods of ascertaining the quantity of chlorine in the sublimate, appear to me at least as objectionable as his process for determining the composition of phosphoric acid, and liable to great errors: the first from the uncertainty of the absolute quantity of chlorine admitted; and the second, from the loss arising from the vapour of the sublimate, which must be carried off by the current of chlorine. How great a deficiency may originate from the last circumstance, is shown by the following experiment: 5 grains of phosphorus were converted into sublimate by chlorine in great excess, the remaining chlorine was displaced by passing common air through the vessel for some time, till not the slightest smell of chlorine could be perceived; the retort was then weighed, and a current of air passed through it. Though this current could hardly have replaced the air contained in the retort, yet the loss of weight was 1.7 grains, and copious vapours were produced in the atmosphere. In a second trial of the same kind, there was a greater loss of weight, and by barely exhausting the retort, and then again admitting air, there was a loss of $\frac{7}{10}$ of a grain.

When chlorine is made to act upon phosphorus over mercury not carefully dried, some muriatic acid gas is always formed; but when the mercury has been recently boiled, no effect of this kind is produced, and the vapour in the gas forms a minute quantity of a liquid hydrate of the perchloride, which by more water, is converted into muriatic and phosphoric acids, as I proved by some very delicate experiments; so that there is certainly no hydrogen denoted in phosphorus by the action of chlorine, and in their mutual action a mere binary compound of the two substances is formed.

After reflecting much upon the methods of combining chlorine and phosphorus, so as to gain correct results, it occurred to me, that in operating over water, and introducing a perfectly saturated solution of chlorine to absorb the vapour of the sublimate and of its hydrate formed from the water in the chlorine, I should gain a result nearly correct. I made an experiment in this way on 4 grains of phosphorus, in a retort containing 13 cubical inches. I ascertained the absorption, introduced into the retort a tube, containing about half a cubical inch of saturated solution of chlorine, and suffered the fluid slowly to act upon the sublimate, cooling the retort by immersion in water; I then ascertained the degree of the second absorption, which was nearly a cubical inch and a half. I likewise ascertained that water had its powers of dissolving chlorine diminished, and not increased, by uniting with phosphoric and muriatic acids, so that the apparent absorption must have been less than the real one. Adding the second absorption to the first, and making the proper corrections, the quantity of chlorine uniting to 4 grains of phosphorus was 31.9 cubical inches; barometer being 30.1 inches, and thermometer 46° FAHRENHEIT.

Rather a larger proportion would be given, if the correction for the presence of vapour had been made for some of the other experiments: and the result agrees exactly with the mean deduced from the absorption of oxygen in the formation of phosphoric acid; for, assuming that 100 cubical inches of chlorine weigh 76.5 grains, then the sublimate will consist of 1 of phosphorus to nearly 6 of chlorine; and taking the composition of phosphoric acid from this datum, it would consist of 100 of phosphorus and 135 of oxygen.

To ascertain the composition of phosphorous acid, I used a new method, that of converting the perchloride of phosphorus, or perphosphorane by phosphorus, into the chloride which affords phosphorous acid by the action of water. This is easily done by heating them together in a close retort; and it enables us to determine with certainty, which opinion is correct, *that* assuming the oxygen in phosphorous acid to be 3, or *that* which supposes it to be 2.5, the oxygen in phosphoric acid being 5.

5 grains of phosphorus were converted into perchloride in a small retort of the capacity of 6 cubical inches: it was necessary to exhaust this retort twice to remove the residual common air mixed with the chlorine, and some perchloride must have been lost during this process. A small quantity of chlorine, which could have been little more than sufficient to compensate for the loss, remained in the retort. 5 grains of phosphorus were introduced, and the retort suffered to remain filled, principally with common air; heat was very slowly applied; all the phosphorus, except an atom not so big as the head of a small pin, disappeared, and a little of the sublimate still remained, when the retort burst from the expansion of the vapour of the new chloride formed; but the chloride found on the fragments was pure, and held no phosphorus in solution.

A second experiment was made in a retort of the capacity of 11 cubical inches. 5 grains of phosphorus were converted into perchloride: the retort was twice completely exhausted, by which at least a grain and a half or two grains of perchloride must have been lost. 5 grains of phosphorus were introduced; a little of the sublimate was lost by falling into

the stop-cock of the retort; yet the conversion of the phosphorus by heat into the liquor was almost complete; there remained only a minute fragment. In this experiment, however, the liquor held phosphorus in solution. When this phosphorus was precipitated by water, and obtained with the fragment by sublimation in a small glass tube, it did not equal $\frac{7}{10}$ of a grain, and was no more than could be expected from the loss of the sublimate.

These two experiments prove distinctly that the oxygen in phosphorous acid is half that in phosphoric acid; for if the proportion had been that which M. DULONG and M. BERZELIUS indicate, 1.67 grains of phosphorus, at least, ought to have remained after the action of the sublimate.

A collateral experiment was made. 32.7 grains of the fluid chloride, made by passing phosphorus through corrosive sublimate in great excess, were acted on by water, and precipitated by nitrate of silver; the precipitate was immediately separated from the fluid, after it had been greatly diluted with distilled water. Distilled water was then repeatedly passed through it, and it was dried and fused, when it weighed 98.4 grains; which, allowing 24.5 per cent. of chlorine in horn silver, would give the composition of the fluid chloride as 24.108 of chlorine, and 8.592 of phosphorus.

The comparative quantity of precipitate in this experiment was so much less than I had found in a former experiment, that, notwithstanding the care with which the process had bee conducted, I resolved to make some more experiments of the same kind. In the first, in which the decomposition by water was made in a small bottle, from which no vapour could escape, and in which I superintended the weighing and drying

of the horn silver formed, with the greatest care, 18.4 of the liquid chloride afforded only 54.5 of chloride of silver, which agrees as nearly as could be expected with the former experiment. In two other experiments, made with equal care, and in which the liquid was poured into a solution of nitrate of silver, 6 grains gave 17.1 of horn silver, and 29.4 gave 89.9 of fused horn silver.

In examining minutely the circumstances of the action of the liquid chloride, or solutions containing phosphorous and muriatic acids, or nitrate of silver, I found no difficulty in explaining the cause of the error in the former experiments. Phosphorous acid acts upon nitrate of silver, and more rapidly in proportion to its concentration, and gradually produces a copious precipitate from it; so that if there be an excess of nitrate of silver, and the precipitate be not immediately separated from the solution, there is always a considerable increase of weight. M. Dulong, and M. Berzelius, whose experiments agree with my former ones, *may* have been misled by a precipitation from the nitrate of silver by phosphorous acid, as I am sure *I* was. M. Berzelius does not state how he prepared his liquid chloride of phosphorus; but M. Dulong, who objects to my process by corrosive sublimate, and employs, instead of it, the action of chlorine on phosphorus in forming his fluid, must have been exposed to other sources of error. He speaks of acting on dry phosphorus by dry chlorine; but it must be always extremely difficult to free a gas that cannot be kept over mercury, of all its vapour; and as perchloride always forms during the action of phosphorus on chlorine, a part of which produces a fluid, and easily volatile hydrate with water, and soluble in

ll proportions in the liquid chloride, this process must be very liable to error. I have never been able to form the perchloride, even from chlorine slowly passed through muriate of lime, without producing a small quantity of liquid hydrate of perchloride, which, when the solid perchloride was converted into liquid by more phosphorus, rose in vapour with it, and which, containing nearly a double quantity of chlorine, (for the water forms a very small part of it) occasions the precipitation of a much larger quantity of horn silver than the pure chloride formed from corrosive sublimate.

These various experiments on the combination of phosphorus with oxygen and chlorine, sufficiently agree with each other to afford the means of determining the proportion in which phosphorus combines with other bodies, or its equivalent number considered as an element.

If the absorption of oxygen be considered as offering the data, and phosphoric acid be supposed to consist of two proportions of oxygen, and one of phosphorus, the number representing the proportion in which phosphorus combines, will be 22.3. If phosphoric acid be considered as consisting of four proportions of oxygen, the proportional number or equivalent of phosphorus will be 44.6.

If the absorption of chlorine in forming phosphorane be made the datum, the number will be the same, 22.2, or the double 44.4. If the quantity of horn silver formed from the liquid chloride, taking the mean of all the experiments, be assumed as the datum, the number would be 23.5, or the double 47: the mean of all these proportions is 22.6, or the double 45.2; or taking away decimals, 45.

In referring to the analyses which have been made of the

different combinations of phosphoric acid, for the purpose of ascertaining if they correspond with this number, I found the data so uncertain and so discordant, that it was impossible to form any conclusions from them. The phosphate of soda, as is well known, has alkaline properties; yet, according to M. Berzelius, it contains but 17.67 of soda to 20.33 of acid; whereas it ought to contain, according to the proportion indicated by my experiments (if neutral) nearly an equal weight of soda. M. Berzelius mentions several combinations of baryta and lime with phosphoric acid, of which only two approach to a correspondence with the number I have given for phosphorus; that containing 45.5 of acid to 48.7 of lime; and that containing 39.1 of acid to 60.8 of barytes. New researches are required to explain the anomalies presented by the phosphates.

I shall give three experiments on the quantity of hydrate of potassa necessary for saturating given quantities of phosphoric acid made from given weights of phosphorus.

18 grains of phosphorus converted into phosphoric acid by combustion in oxygen, required for its saturation 47 grains of dry hydrate of potassa.

5.7 grains of phosphorus converted into acid, required 14.7 of hydrate of potassa.

5 grains of phosphorus converted into perchloride, demanded, to produce perfect neutralization, 68 grains of hydrate of potassa.

These three experiments agree so well with each other, and with the proportionate number gained from the absorption of chlorine and oxygen by phosphorus, that it is impossible not to put confidence in them.

If 13.1 be considered as the quantity of hydrate of potassa required to neutralize the phosphoric acid formed in the last experiment, and the 54.9 of hydrate remaining, be supposed to contain 43 grains of potassa, then the chlorine required to expel the oxygen from the potassa would be rather more than 40 cubical inches.

We owe to the ingenuity of M. DULONG the discovery of an acid, which he names the hypophosphorous acid, and which he supposes to contain half the quantity of oxygen in the phosphorous acid. I have satisfied myself as to the correctness of his views respecting the existence of this acid, and the properties of its compounds; but I cannot regard the method he has adopted for its analysis as entitled to confidence. He takes a given quantity of hypophosphite of soda, acts upon this by chlorine, converts the excess of chlorine into muriatic acid, precipitates by nitrate of silver and earthy salts, and from the comparison of all these data, in which some substances of uncertain composition may be concerned, draws his conclusions.

I have found that the neutral hypophosphite of barytes, when acted on by heat in close vessels, is converted into acid phosphate of barytes, disengaging an elastic fluid, which is almost entirely the hydrophosphoric gas, or phosphuretted hydrogen saturated with phosphorus. I say *almost entirely*, because in the beginning of the process, a little gas spontaneously inflammable is produced, and a minute quantity of moisture appears: and when the heat is raised to redness, a very little phosphorus is produced, probably from the decomposition of a part of the phosphoric gas. Now supposing the quantity of phosphoric acid in phosphate of baryta known,

and the quantity of phosphorus in phosphuretted hydrogen known; it is very easy, from an accurate experiment on the decomposition of the hypophosphite of baryta, to learn the composition of hypophosphorous acid.

I made two experiments on this subject; in one, 50 grains of dry hypophosphite of barytes were used, and the distillation conducted in a small glass tube. About 29.25 cubical inches of gas were produced. The loss of weight of the apparatus could not be ascertained, as unluckily a little of the phosphate was lost; a small portion of phosphorus was deposited in the upper part of the tube, from the decomposition of a minute quantity of the bi-phosphuretted gas; but this could not have equalled the $\frac{4}{10}$ of a grain, as the tube only lost $\frac{4}{10}$ by being heated to whiteness.

In the second experiment, 29 grains of the hypophosphite were used, and the loss of weight only ascertained, which was 3.5 grains. To be able to form any opinion as to the composition of the hypophosphorous acid, it was necessary to ascertain the composition of the phosphate of baryta produced in these experiments; which was easily done by precipitating a given quantity of the hypophosphite of barytes by sulphate of soda in solution. 15 grains of hypophosphite of barytes, in an experiment very carefully made, afforded 11.3 of sulphate of barytes. Now, supposing this sulphate of barytes to contain 7.4 of baryta, the hypophosphite would consist of 7.4 of baryta, and 7.6 of hypophosphorous acid; and 18.1 of the acid phosphate of baryta, formed from its decomposition, would contain 5.7 phosphoric acid, and 7.4 baryta. And in the experiment in which 29 grains of hypophosphite of baryta were decomposed, supposing the whole

loss of weight to be owing to perphosphuretted hydrogen given off, and this gas to be composed of 92.5 of phosphorus to 4 of hydrogen, or of 5.29 hydrogen to 29.76 phosphorus, and the 25.5 of acid phosphate remaining composed of 14.47 baryta nearly, and 11.03 phosphoric acid, adding the 29.76 of phosphorus to the 4.72 in the phosphoric acid, and subtracting 39, the quantity of oxygen required to form water with the 5.24 of hydrogen, the hypophosphorous acid may be conceived to be composed of 7.69 phosphorus, and 2.54, which denotes rather less than half the oxygen in phosphorous acid: i. e. as 7.48 to 1.5, an approximation nearer than could have been expected.

Assuming the composition of the phosphuretted gas to be what is stated in the preceding page, which agrees very nearly with an experiment which I formerly made, the first experiment on the quantity of gas disengaged would give a proportion of oxygen rather less than that which has been just calculated upon; but it must be remembered, that a certain quantity of common phosphuretted hydrogen is produced, which containing less hydrogen in a given volume, would sufficiently explain the difference of result.

M. DULONG has advanced an ingenious opinion, that the hypophosphorous acid *may be considered* as a triple compound of hydrogen, oxygen, and phosphorus. There is another view which may be taken of its composition, namely, that it may be a compound of phosphoric acid and perphosphuretted hydrogen. Phosphuretted hydrogen, as may be deduced from some experiments of M. DULONG, has the properties of a very weak alkali; and when expelled from the neutral hypophosphites, they become acid. This view agrees very well with the equi-

valent, or proportional numbers, which represent phosphoric acid and phosphuretted hydrogen. If it be adopted, the hypophosphites must be considered as triple compounds, analogous to the salts containing fixed alkali and earths, or ammonia and earths combined with acids.

M. Dulong imagines that the acid formed by the slow combustion of phosphorus in the air, and which I have supposed to be a mixture of phosphorus and phosphoric acids, is a peculiar acid, a chemical compound of phosphorous and phosphoric acids, which he names phosphatic acid. I cannot say that his arguments give much probability to this opinion. This substance has no crystalline form, no marked character which distinguishes it from a mere mixture of phosphorous and phosphoric acids; and as far as my experiments have gone, it is far from uniform in its composition; and phosphorous and phosphoric acids mixed together, produce a substance of exactly the same kind.

That a mixture of phosphorous and phosphoric acids should be produced by the slow combustion of phosphorus, is not surprising, when it is considered that this phenomenon is connected with different chemical processes, viz. the action of the vapour of phosphorus upon air, the action of solid phosphorus upon the elastic atmosphere, and upon the air dissolved in the moisture attracted by the acids formed; and, unless vapour be present in the air, the process of the slow conversion of phosphorus into acids soon stops.

I have mentioned in the paper to which I have referred, in the beginning of this communication, that the hydrophosphorous acid is decomposed by heat; and that phosphoric acid, and perphosphuretted hydrogen are the results. In examining the nature of the phosphoric acid formed, I find that

it contains water, so that it is a hydrated phosphoric acid. In carefully conducting the experiment, I find likewise, that a small proportion of water is given off with the perphosphuretted gas. I shall give the results of an experiment: 17.5 grains of hydrophosphorous acid were decomposed by heat in a small glass retort carefully weighed; 6.5 cubical inches of elastic fluid were generated, and the loss of the retort was 4 grains. Now, if it be assumed that the hydrate of phosphoric acid * remaining equalled 13.5 grains, and that it contained, according to the law of definite proportions, 1.88 of water, and that the bi-phosphuretted gas weighed 1.937, and consisted of 1.6446 phosphorus, and .2924 hydrogen; then the oxygen in the phosphorous acid will be to the phosphorus as 44 to 66, which is as near a result as can be expected.

For 4 proportions of phosphorous acid are - -	300 or the double	150
and 10 of water - -	170 or	85
which together amount to -	470 or	235
which form 3 proportions of phosphoric acid	315 or	157.5
with 3 of water to form the hydrate -	51 or	25.5
	366	183.0
4 of water decomposed, of which the hydrogen is 8, to form with 45 of phosphorus phosphuretted hydrogen - - -	53 or	26.5
3 of water given off - - -	51 or	25.5
making - - - -	470 or	235.

* I proved it to be a hydrate by heating it with magnesia, when abundance of water was given off from it.

I have no doubt that the acid which I used formerly was drier than the acid employed in this experiment, which will account for the difference of the result. Supposing a hydrophosphorous acid could be procured, containing only the quantity of water sufficient to convert it into dry phosphoric acid, it would consist, as I have stated in my former paper on phosphorus, of four proportions of water, and four proportions of phosphorous acid.

I have adopted throughout the whole of these calculations, the supposition that the hydrogen in water is to the oxygen as 2 to 15; and consequently I have taken the number representing oxygen as 15, which is extremely convenient, as the multiples are simple, 30, 45, 60, &c. Taking the proportion of phosphoric acid in phosphate of potassa, which may be deduced from the experiments, page 329, it appears more convenient to represent the proportional number, or equivalent of phosphorus, by 45, or 45.2, than by 22.5, or 22.6, which gives facility in adopting either hypothesis of the composition of hypophosphorous acid. If it be supposed a simple compound of oxygen and phosphorus, the series of proportions in the acids of phosphorus will be

Hypophosphorous acid,	Phosphorus 45	Oxygen 15
Phosphorous acid	- 45	Oxygen 30
Phosphoric acid	- - 45	Oxygen 60
or hypophosphorous acid 263	Phosphoric acid 2 proportions 210 Phosphuretted hydrogen 1 prop. 53	

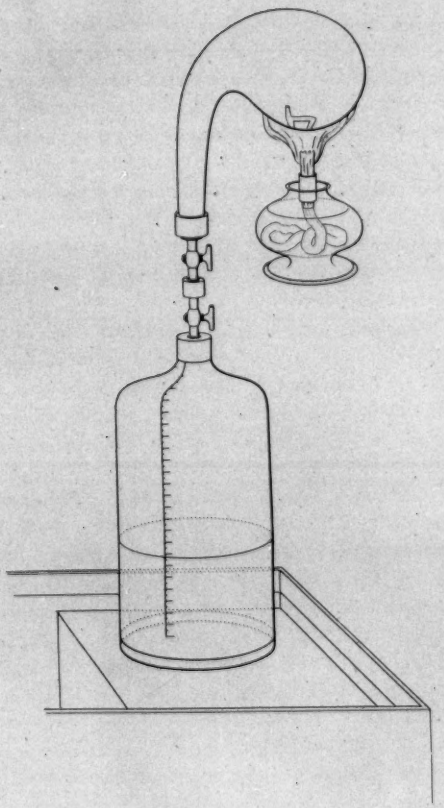
I shall conclude this paper by a few incidental observations on the compounds of phosphorus.

M. Dulong states that no phosphorous acid is formed when phosphorus is burnt in excess of oxygen or atmospheric air;

as, he says, I have asserted. I cannot find that I have any where made such an assertion; but notwithstanding what M. DULONG pretends, the assertion is true, as the following experiment will prove. Half a grain of phosphorus was set fire to in a retort containing 16 cubical inches of common air; the acid products were washed with distilled water, and passed through a filter, and evaporated. When the acid became nearly dry, small globules of phosphuretted hydrogen were disengaged from it, indicating the presence of phosphorous acid. The experiment was repeated two or three times, care being taken to separate the red powder which has been considered as an oxide of phosphorus, and always with the same result.

Whenever phosphorus is inflamed, and suffered to become extinguished in oxygen gas in excess, unless the *product* is strongly heated after the spontaneous combustion is over, *an acid*, of which the hydrate produces phosphuretted hydrogen by heat, is always found in the products; and this acid is probably produced by the action of the solid phosphorus on the phosphoric acid in contact with it. This fact, and the circumstance, that much phosphorus acid is produced by the combustion of phosphorus in rare air, renders it almost certain that the phosphorous acid is a direct combination of phosphorus and oxygen, and destroys an idea which might otherwise be formed from the phenomena of the decomposition of its hydrate, namely, that it is a compound of three proportions of phosphoric acid, and one of phosphuretted hydrogen.

M. DULONG and M. BERZELIUS speak of freeing phosphorane, or the liquid chloride of phosphorus, from phosphorus,



by distillation. In experiments made in the laboratory of the Royal Institution, in which it has been twice carefully distilled at a low heat, it has still contained minute quantities of phosphorus.

It has been supposed that dry phosphoric acid is fixed at a white heat; but I find that this is not the case: it rapidly rises in vapour at this temperature, and evaporates even at the point of fusion of flint glass: and the hydrate of phosphoric acid is susceptible of being volatilized at a much lower temperature.

In converting the solid sublimate composed of phosphorus and chlorine into the liquid compound, when the phosphorus is first fused in contact with the sublimate, a yellow crystalline mass is formed, which, when acted on by a higher degree of heat, affords the liquid chloride, which rises from it in vapour, and leaves phosphorus behind. It is possible that this yellow solid is a compound of phosphorus and chlorine, containing half as much chlorine as the liquid. Should this be proved to be the case by future experiments, it will give weight to the idea, that the hypophosphorous acid is a binary compound of oxygen and phosphorus.

XVIII. *New experimental researches on some of the leading doctrines of caloric; particularly on the relation between the elasticity, temperature, and latent heat of different vapours; and on thermometric admeasurement and capacity.* By Andrew Ure, *M. D. Communicated by* W. H. Wollaston, *M. D. F. R. S.*

Read April 30, 1818.

Glasgow, July 1817.

1. *On the elastic force of vapours, with new formulæ to determine it at any temperature; and a review of those given by* Dalton *and* Biot.

The phenomena attending the conversion of liquids into elastic fluids, were first accurately investigated by Dr. Black. He observed in the rising of vapour, and melting of ice, a beautiful system of relations, connecting and modifying the grandest operations of nature, while they were destined to afford new principles for the advancement of the arts. If it be the prerogative and characteristic of genius, to discover in the most familiar, or, as some would say, vulgar phenomena, that mystic chain of causation, which had eluded all other eyes, unquestionably, the doctrines of latent heat entitle their author to rank in the first class of philosophers.

Dr. Black directed his attention principally to the establishment of the general laws, which he placed on an immoveable basis; leaving to his pupils, the subordinate task of investigating their individual applications. Hence, the elastic forces of the vapours, arising from different bodies, at different temperatures, seem to have occupied him very little, if at all.

This subject was examined, however, with great ability, by two of his most distinguished friends, Professor ROBISON and Mr. WATT. The investigations of the former were published in the Encyclopædia Britannica, article *steam*; while we have still to regret our ignorance of those executed by the latter philosopher, with probably a more complete apparatus, and more extensive views. We are indebted to him, indeed, for some curious observation on the latent heat of steam, at different temperatures, which make us lament more, the want of those on the elastic forces themselves.

Mr. DALTON, whose peculiar speculations on caloric and meteorology led him to study the formation and variable elasticity of vapour with great attention, has since then favoured the world with many excellent dissertations, and is now reckoned the first authority on the subject. Mr. DALTON's experiments on the steam of water were carried no higher than its ordinary boiling point; but from the observed progression of its elastic force he investigated a formula, and calculated from it a table for the higher temperatures.*

In the second number of the *Journal Polytechnique*, M. BETANCOURT, an eminent Spanish engineer, long resident at Paris, published a set of experiments on the same subject, the results of which differ from those of Mr. DALTON in many particulars, but most remarkably in the higher part of the scale.

Having had my mind often called to this important inquiry in the course of my public lectures on the applications of Science to the Arts, an apparatus of a very simple nature occurred to me, about two years ago, by which I hoped to be

* Manchester Memoirs, vol. v. p. 563.

able to determine, with great precision, the elastic forces of vapours at any temperature, from zero of FAHRENHEIT, to a much higher degree of heat than even BETANCOURT seems to have reached. The experiments were made soon after that time, but circumstances have till now prevented me from arranging them for publication.

With BETANCOURT's apparatus I am not acquainted, having seen only the brief table of results, inserted in our systematical works on chemistry. Professor ROBISON's consisted of a strong boiler or digester, containing the water, and furnished with three small apertures; the first receiving the bulb of a thermometer, the second covered with a safety valve, and the third having a barometer tube attached. At first I used a similar construction, but finding it hazardous, and somewhat unmanageable in the high heats, and difficult to render air tight in the lower temperatures, I abandoned it, after some unsatisfactory trials. At the low degrees of heat, the vacant part of the barometer tube introduces errors, since it has not the temperature of the boiler; and the bulb of the barometer, used in high heats, occasions a similar fallacy in the determination of the true elasticities.

Still, however, it was ingeniously conceived, and the results furnish good approximations, creditable to the celebrated experimenter.* They agree nearly with those of BETANCOURT, being obtained probably in a similar way. The method adopted by Mr. DALTON, is recommended by an elegant simplicity. It is merely a common barometer, into which a little of the vapour-giving liquid is introduced, so as to moisten, and float above the mercury. The vapour which is

* See Encyclopædia Britannica, vol. xvii. p. 739, 2d Edition.

generated, depresses more or less the barometric column. Hence, by subjecting the liquid to successive degrees of temperature, the corresponding depressions of the barometer, or elasticities of the vapour, are obtained.

The only difficulty in this mode of operating, is to bring a considerable length of vertical tube to an uniform temperature. Mr. DALTON, well aware of this source of error, obviated it in a great measure, by taking a series of different tubes, decreasing in their lengths with the increasing expansions of the vapour, and concomitant descent of the mercurial column. In several experiments conducted on this plan, I found it scarcely possible to obtain results rigidly corresponding with each other, when the column of vapour, exposed in the barometer tube to the influence of surrounding heat, exceeded two inches in length.

M. BIOT, in his system of physics recently published, while he adopts Mr. DALTON's results as the basis of his reasoning, treats fully of this difficulty, and suggests an ingenious means of avoiding it. "We have had occasion several times to "remark," says he," that the temperature of a mass of liquid "which cools in the air, is not entirely the same at the bottom, "as it is at the top of the vessel; because the colder particles "subside into the lower strata, by the excess of their weight. "Thus the temperature of the column of hot water, which "surrounds the tube in the preceding experiment, cannot "be rigorously uniform throughout its whole height. We "may endeavour to render it equal, by agitating and mingling "the different strata of which it is composed; but this would

" be attended with no little difficulty. It would be better to " have several thermometers suspended at different heights, " in the body of the water, and to take the arithmetical mean " of their indications. Or otherwise, which would probably " be more exact, we might employ a thermometer having a " cylindrical bulb, equal in length to the column of vapour. " It would then be necessary that the column of water should " rise sufficiently above this vapour to allow the thermo- " meter bulb to be equally immersed, or we must make on " its indications the small correction mentioned p. 59, in " order to reduce the temperature of the cylinder of mercury, " to the temperature of the reservoir. The employment of " such a thermometer may appear at first sight sufficiently " difficult, since it seems that the length of the cylindrical " reservoir must be very considerable, if the elastic force of " the vapour be great.*"

He then proceeds to show how this difficulty may be obviated (as indeed it had previously been by Mr. DALTON), by taking barometer tubes successively shortened, as the force of the steam is augmented by heat. He proposes to use four, between the freezing and boiling points of water, each being two decimeters, or nearly 8 inches long, and the thermometer bulb having also that length. The plan which I imagined, as it completely obviates the source of errors arising from the large and variable space occupied by the vapour, supersedes the necessity of employing M. BIOT's singular remedy. It likewise avoids other complications, introduced by the heating and consequent elongation of the mercurial column itself

* Traité de Physique. Tome I. p. 268.

attending all the other methods; and scarcely capable of being exactly appreciated at high temperatures with the apparatus of Professor Robison.

The space over which the vapour extends in my instrument, need never be greater than half an inch of a barometer tube, against the side of which part the oblong bulb of a delicate thermometer rests, so as to indicate the true temperature. And though the liquid and incumbent vapour are thus always restricted to the summit of the barometer tube, we can, notwithstanding, measure its progressive range of elasticity, from zero of Fahrenheit to one hundred, or even two hundred, degrees above the boiling point of water, from an elasticity of 0.07 of an inch, to that capable of sustaining 14 feet, or even 36 of mercury. Fig. 1 (Pl. XIX.) represents the construction employed for temperatures under and a little above the boiling point. Fig. 2 and 3 are used for higher temperatures; the last is the more convenient of the two. Each was suspended from a lofty window ceiling, and placed in a truly vertical position by means of a plumb line.

One simple principle pervades the whole train of experiments; which is, that the progressive increase of elastic force developed by heat from the liquid, incumbent on the mercury at $l\ l'\ l''$, is measured by the length of column which must be *added* over L, the primitive level below, in order to restore the quicksilver to its primitive level above, at l. These two stations, or points of departure, are nicely defined by a ring of fine platina wire twisted firmly around the tube.

At the commencement of the experiment, after the liquid well freed from air has been let up, the quicksilver is made a tangent to the edge of the upper ring, by cautiously pouring

mercury in a slender stream into the open leg of the syphon D. The level ring below is then carefully adjusted.

From the mode of conducting my experiments, there remained always a quantity of liquid in contact with the vapour, a circumstance essential to accuracy in this research.

Suppose the temperature of the water or the oil in A to be 32° F., as denoted by a delicate thermometer, or by the liquefaction of ice; communicate heat to the cylinder A, by means of two argand flames, playing gently on its shoulder at each side. When the thermometer indicates 42°, modify the flames or remove them, so as to maintain an uniform temperature for a few minutes. A film or line of light will now be perceived between the mercury and the ring at *l*, as is seen under the vernier of a mountain barometer when it is raised a few feet off the ground. Were the tube at *l* and L of equal area, or were the relation of the areas experimentally determined, then the rise of the quicksilver above L would be one half, or a known submultiple of the total depression, equivalent to the additional elasticity of the vapour at 42° above that at 32°. Since the depressions, however, for 30 or 40 degrees in this part of the scale are exceedingly small, one half of the quantity can scarcely be ascertained with suitable precision, even after taking the above precautions. And besides, the other sources of error, or at least embarrassment, from the inequalities of the tube, and from the lengthening space occupied by the vapour, as the temperature ascends, render this method of reduction very ineligible.

By the other plan we avoid all these evils. For whatever additional elasticity we communicate to the vapour above *l*, it will be faithfully represented and measured, by the

mercurial column which we must add over L, in order to overcome it, and restore the quicksilver under *l* to its zero or initial level, when the platina ring becomes once more a tangent to the mercury.*

At E a piece of cork is fixed, between the parallel legs of the syphon, to sustain it, and to serve as a point by which the whole is steadily suspended.

For temperatures above the boiling point, the part of the syphon under E is evidently superfluous, merely containing in its two legs a useless weight of equipoised mercury. Accordingly for high heats, the apparatus fig. 2, or 3, is employed, and the same method of procedure is adopted. The aperture at O, fig. 3, admits the bulb of the thermometer, which rests as usual on *l''*. The recurved part of the tube is filled with mercury, and then a little liquid is passed through it to the sealed end. Heat is now applied by an argand flame to the bottom of C, which is filled with oil or water, and the temperature is kept steadily at 212° for some minutes. Then a few drops of quicksilver may require to be added to D'' till L'' and *l''* be in the same horizontal plane. The farther conduct of the experiment differs in no respect from what has been already described. The liquid in C is progressively heated, and at each stage mercury is progressively added over L'' to restore the initial level, or volume at *l''*, by equipoising the progressive elasticity. The column above L'' being measured, represents the succession of elastic forces. When this column is wished to extend very high, the vertical tube requires to be placed for support in the groove of a long wooden prism.

* Rings of other metals will not suit; for their expansions being much greater than that of glass, they become loose with the elevation of temperature.

The height of the column in some of my experiments being nearly 12 feet, it became necessary to employ a ladder to reach its top. I found it to be convenient in this case, after observing that the column of vapour had attained its primitive magnitude, to note down the temperature with the altitude of the column; then immediately to pour in a measured quantity of mercury nearly equal to three vertical inches, and to wait till the slow progress of the heating again brought the vapour in equilibrio with this new pressure, which at first had pushed the mercury within the platina ring at *l''*. When the lower surface of the mercury was again a tangent to this ring, the temperature and altitude were both instantly observed.

This mode of conducting the process will account for the experimental temperatures being very often odd and fractional numbers. I present them to the public as they were recorded on the instant in that particular repetition of the experiment which I consider most entitled to confidence. To trim and fashion the results into an orderly looking series, would have been an easy task; but in my opinion this is a species of deception very injurious to the cause of science, and a deviation from the rigid truth of observation, which ought never to be made for any hypothesis. We shall afterwards have ample opportunities of exposing the fallacy of such premature geometrical refinements.

The thermometers were constructed by CREIGHTON, with his well known nicety, and the divisions were read off with a lens, so that $\frac{1}{10}$ of a degree could be distinguished. After bestowing the utmost pains in repeating the experiments during a period of nearly two months, I found that the only way of removing the little discrepancies, which crept in

between contiguous measures, was to adopt the astronomical plan of multiplying observations and deducing truth from the mean. It is essential to heat with extreme slowness and circumspection, the vessels, A, B, C. One repetition of the experiment occupies on an average 7 hours.

TABLE I.

The elastic force of the vapour of water in inches of mercury.

Temp.	Force.	Temp.	Force.	Temp.	Force.	Temp.	Force	Temp.	Force.	Temp.	Force.
24°	0.170	115°	2.820	195°	21.100	242°	53.600	270	86.500	295.6	130.400
32	0.200	120	3.300	200	23.600	245	56.340	271.2	88.000	295	129.000
40	0.250	125	3.850	205	25.900	245.8	57.100	273.7	91.200	297.1	133.900
50	0.360	130	4.366	210	28.880	248.5	60.400	275	93.480	298.8	137.400
55	0.416	135	5.070	212	30.000	250	61.900	275.7	94.600	300	139.700
60	0.516	140	5.770	216.6	33.400	251.6	63.500	277.9	97.800	300.6	140.900
65	0.630	145	6.600	220	35.540	254.5	66.700	279.5	101.600	302	144.300
70	0.726	150	7.530	221.6	36.700	255	67.250	280	101.900	303.2	147.700
75	0.860	155	8.500	225	39.110	257.5	69.800	281.8	104.400	305	150.560
80	1.010	160	9.600	226.3	40.100	260	72.300	283.8	107.700	306.8	154.400
85	1.170	165	10.800	230	43.100	260.4	72.800	285.2	112.200	308	157.700
90	1.360	170	12.050	230.5	43.500	262.8	75.900	287.2	114.800	310	161.300
95	1.640	175	13.550	234.5	46.800	264.9	77.900	289	118.200	311.4	164.800
100	1.860	180	15.160	235	47.220	265	78.040	290	120.150	312	167.000
105	2.100	185	16.900	238.5	50.300	267	81.900	292.3	123.100	Another expert.	
110	2.456	190	19.000	240	51.700	269	84.900	294	126.700	312°	165.5

The apparatus employed in obtaining these results, has the peculiar advantage over all others, that the mercurial column is never heated. It is the concurrent opinion of all chemical philosophers, that caloric travels downwards in liquids with extreme slowness and difficulty. Indeed, Count RUMFORD's experiments led him to infer that heat could not descend in fluids at all.

It is evident that in my constructions, figures 1, 2, and 3, only that small portion of quicksilver, within the vessels

A, B, and C, will be affected by the heat, but the measuring column is beyond the reach of its influence.

A surprising accordance will be perceived between my numbers, and those given by Mr. DALTON between 32° and 212°, though mine were obtained with a different modification of apparatus. Above the boiling point, where the table of Mr. DALTON is deduced from calculation, the accordance soon ceases. But as my apparatus and mode of using it were precisely the same as in the former part of the range, my results, if entitled to confidence in the one case, must be so in the other. At 280° BETANCOURT's number and mine are not much different, the former being 105 inches, the latter 102. Being perfectly convinced, by repeating the experiments in different circumstances, that Mr. DALTON's ratio of progression, though apparently accommodated to the intervals between 32° and 212°, could not serve for the higher ranges,* I endeavoured to discover a simple rule of more general application. It is above 212°, indeed, that for the purposes of art, the knowledge of the force of steam is required.

I first tried the differential method, so useful for determining the distant links of a concatenated series.

Without doing much violence to the above numbers, the forces corresponding to 100°, 110°, 120°, 130°, 140°, and 150°, may be written in a series of which the 5th order of differences = 0. Then if d' d'' d''' d^{iv} d^{v}, represent the first terms, in the first, second, third, fourth, and fifth order of differences, the n^{th} term of the series is

* Dr. YOUNG remarks on DALTON's ratio, "It is certain that this cannot be the law of nature, since about 394° the elasticity would become uniform, and then decrease, if the law be true." YOUNG's Natural Philosophy, 4to. vol. ii. p. 398.

$a + \overline{n-1} \cdot d', + \overline{n-1} \cdot \frac{n-2}{2} \cdot d'', + \overline{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot d''', +$
$\overline{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot \frac{n-4}{4} \cdot d'''', +$ &c.

In the above series for steam, $d' = 0.65$, $d'' = 0.19$, $d''' = 0.04$, $d^{iv} = 0.01$, $d^{v} = 0$. $a = 1.92$.

Example 1st. To determine the 8th term in the series, or the elastic force at 8×10, above 90°, (the first term 100° being included) or at 170°.

Here $n = 8$

$a + \overline{n-1} \cdot d' = 1.92 + 4.55 =$	6.47
$\overline{n-1} \cdot \frac{n-2}{2} \cdot d'' =$	4.08
$\overline{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot d''' =$	1.40
$\overline{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot \frac{n-4}{4} \cdot d^{iv} =$	0.35
	12.30

Observation gives 12.05, forming a good accordance.

Example 2. Required the 10th term, or $n = 10$. For 190° F.

$a + \overline{n-1} \cdot d' =$	7.77
$\overline{n-1} \cdot \frac{n-2}{2} \cdot d'' =$	6.84
$n-1 \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot d''' =$	3.36
$n-1 \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot \frac{n-4}{4} \cdot d^{iv} =$	1.26
	19.23

At 190° experiment makes it 19.00, still coinciding nearly.

By the same equation we find the 20th term or for 290° to be 124.28, while experiment gives 120.15, showing a difference of 4.13 inches. At a higher point the error becomes greater. We here see that a geometrical series may coincide

apparently through a considerable range with experiment, and yet be inaccurate when farther extended.

Dissatisfied, therefore, with this approximation, I prosecuted the inquiry, and had the happiness to discover a very simple and beautiful ratio, which will actually apply through an extensive scale of temperature, and is incomparably easier in practice than the preceding rule. The elastic force at 212° = 30 inches being divided by 1.23, will give the force for 10° below; this quotient divided by 1.24, will give that for 10° lower; and so on progressively. To obtain the forces above 212°, we have merely to multiply 30 by the ratio, 1.23 for the force at 222°; this product by 1.22 for that at 232°, and thus for each successive interval of 10° above the boiling point.

Thus $30 \times 1.23 = F_{212°}$. $30 \times 123 \times 1.22 = F_{232°}$, using F to denote the force at any temperature n, according to the notation of LAPLACE.

By departing from the point of 210° F., we shall obtain results equally accurate, but more convenient for comparison with the experimental table. The following numbers exhibit the correspondence of this ratio with actual observation.

Table II.

Observed elasticity of aqueous vapour compared with the ratios.

Temp.	Calcul. Force.	Expert.	Temp.	Calcul. Force.	Expert.	Dalton.	Betanc.	Robison.
210°	28.9	28.9	210°	28.9	28.9	28.84	28.8	28.65
200	23.5	23.6	220	35.54	35.54	34.99		35.80
190	19.0	19.0	230	43.36	43.10	41.75	45.5	44.70
180	15.2	15.16	240	52.46	51.70	49.67		54.90
170	12.07	12.05	250	62.95	61.90	58.21		66.80
160	9.50	9.60	260	74.91	72.30	67.73	80.17	80.30
150	7.42	7.58	270	88.39	86.30	77.85		94.10
140	5.75	5.77	280	103.41	101.90	88.75	105.12	105.90
130	4.42	4.36	290	119.95	120.15	100.12		
120	3.37	3.33	300	137.94	139.70	111.81		
110	2.55	2.45	310	157.25	161.30	123.53		
100	1.92	1.86	320	177.70		135.00		
90	1.43	1.36						
80	1.06	1.02				Temp.	Betanc.	Robison.
70	0.77	0.726						
60	0.56	0.516				32°	0.0	0.0
50	0.40	0.36				50		0.12
40	0.28	0.25				80	0.81	0.82
30	0.20	0.19				100	1.65	1.60
20	0.14	0.14				120	2.95	3.00
10	0.098					140	5.00	5.15
0	0.068					160	9.00	8.65
						180	14.00	14.05
						200	22.50	22.62

The rule on which the preceding table is formed, may be expressed in a manner better fitted to give *directly* the elastic force corresponding to any given temperature moderately distant from 212°. It becomes also more accurate.

Let r = the mean ratio between 210° and the given temperature; n = the number of terms (each of 10°) distant from 210°; F = the elastic force of steam in inches of mercury.

Then, Log. of F = Log. 28.9 $\pm n$. Log. r; the positive sign being used above, the negative below 210°.

Or by common arithmetic, multiply or divide 28.9, according as the temperature is above or below 210°, by the mean ratio, involved to a power denoted by the number of terms. The product or quotient is the tension required.

Example 1st. The temperature is 140°. What is the corresponding elasticity of the vapour from water heated to that point?

140° is 7 terms of 10° each *under* 210°; 1.26 is the mean ratio $= \frac{1.23 + 1.29}{2}$; and, consequently, $r = 1.26$; $n = 7$.

Log. 28.9 = 1.46090

Log. $1.26 \times 7 = 0.10037 \times 7 = -$ 0.70259

0.75831, which is

the logarithm of - - 5.732 inches.

Experiment gives - - 5.77 , difference .04, inconsiderable.

Example 2. What is the tension of steam at the temperature of 290°?

$r = \frac{1.23 + 1.16}{2} = 1.195$ $n = 8$

Log. 28.9 = 1.46090

8. Log. $r = 8 \times 0.07787$ $= +$ 0.61896

Log. of 140.02 inches 2.07986

At 290° by experiment = 120.15

Example 3. Temperature 250°. Force of steam in contact with water?

$r = \frac{1.23 + 1.20}{2} = 1.215$ $n = 4$

Log. 28.9 = 1.46090

4 Log. $r = 4 \times 0.08458$ $= +$ 0.33832

Log. of 62.98 1.79922

At 250° Experiment 61.90

At these high heats, it is very possible that the experiment may be in error by 1 inch, which is the whole difference here. About half a degree of FAHRENHEIT misnoted, would give this deviation.

Such a correspondence, therefore, of observation with the calculated results, shows that we have found a rule of perfect accuracy for all purposes of engineering, &c. If I am asked whether this formula coincides at every link with the chain of nature, I freely acknowledge, that I do not imagine it strictly so to do. But still it affords approximations such, that within moderate limits, I cannot tell whether to place more confidence in them, or in those found by experiment. It has moreover the rare advantage of being extremely simple, and level to the capacity of all practical men.

In BIOT's excellent work above quoted, where many of the hitherto vague disquisitions of physical science have been happily brought within the pale of geometry, this celebrated philosopher has deduced, from Mr. DALTON's experiments on the force of steam, a general formula for determining its elasticity at any temperature.

In investigating this formula, he represents the decrease of the logarithms of the elastic forces by a series of terms of the form $an + bn^2 + cn^3$; $a\,b\,c$ being constant coefficients.

Thus, Log. $F_n = \text{Log. } 30 + an + bn^2 + cn^3$

It is unnecessary to employ powers of n higher than the cube, because their coefficients would be insensible, as the calculation will show. To determine the coefficients $a\,b\,c$, he makes use of the elastic forces, observed at the temperatures on the centigrade scale of 100°, 75°, 50°, and 25°; whence result these conditions,

$n = 0$ $F = 30.00$ inches
$n = 25$ $F_{25} = 11.25$
$n = 50$ $F_{50} = 3.50$
$n = 75$ $F_{75} = 0.910$

Substituting these conditions in the above general formula, and bearing in mind that the logarithm of a fraction is equal to the logarithm of the numerator minus the logarithm of the denominator, we have the three following equations of conditions.

$$-0.4259687 = 25.a + 625\ b + 15625\ c.$$
$$-0.9330519 = 50.a + 2500\ b + 125000\ c.$$
$$-1.5180799 = 75.a + 5625\ b + 421875\ c.$$

Doubling the first, and subtracting it from the second, a disappears; trebling it, and subtracting it from the third, a also disappears. Then dividing each of the resulting equations by the coefficient of b, we have

$$-0.00006489160 = b + 75\ c.$$
$$-0.00006404635 = b + 100\ c.$$

Subtracting the one of these from the other, b will disappear; and dividing it by the coefficient of c, we shall have c. Next, by substituting the value of c in one of these equations, we get b. Lastly, putting b and c in one of the two first equations, we have a. Thus we find

$$a = -0.01537419550$$
$$b = -0.00006742735$$
$$c = +0.00000003381$$

Whence the whole formula Log. F_n = Log. 30 + an + bn^2 + cn^3 is completely determined, and may serve for calculating F_n, relative to any proposed value of n.

If we make, for example, $n = 100$, we shall have the elastic force at 100 degrees below the boiling point, or at the temperature of melting ice. We thus obtain

Log. $F_n = 1.4771213 - 2.1778831 = -0.7007618$.

Or employing negative indices in order to make use of the ordinary logarithmic tables,

Log. $F_n = \bar{1}.2992382$, whence

$F_n = 0.19917$ inches; and observation gives us 0.200.

The error is obviously insensible; and we may adopt, says M. Biot, our formula as representing the experiments of Mr. Dalton. To introduce the Fahrenheit degrees into the formula, calling them f, and counting from 212°, we have $\frac{5}{9} f = n$; and substituting this value of n in the preceding formula, we obtain

$$a = -0.00854121972$$
$$b = -0.00002081091$$
$$c = +0.00000000580,$$

whence Log. $F_f = 1.4771213 + af + bf^2 + cf^3$, f being the number of degrees of Fahrenheit, reckoning them from 212°, positive below and negative above this point of departure.

By the above formula, thus elaborately investigated by M. Biot, I have computed the elastic forces of steam at the three successive temperatures of 232°, 262° and 312°, or 20°, 50° and 100°, above the boiling point of Fahrenheit's scale.

In the first case we have $f = -20$ and $af + bf^2 + cf^3 = 20 + 400\,b - 8000\,c$; f is negative, being above the point of departure 212°, and, consequently, the products af and cf^3 are positive, while bf^2 becomes negative.

$20\,a = 0.170824$
$400\,b = -\ 0.008324$
$8000\,c = +\ 0.000046$

$0.162546 +$ log. 30 or 1.477121
1.477121

Log. of 43.62 = 1.639667

By Biot's formula therefore at 282 F. - 43.620
My experiments - - - 44.700
Mr. Dalton's table - - - 43.25
Betancourt - - - 47.20

By M. Pouillet's table at the end of Biot's 1st vol.
computed from the above formula - 43.500

The difference between Biot and my experiments here is only 1.10 inches.

2d Example. Temperature 262° Fahr. $f = 50$

$50\,a = 0.4270609$
$2500\,b = -\ 0.0520272$
$125000\,c = +\ 0.0007250$

0.3757587
Log. 30 = 1.4771213

Log. of $F_{262°}$ = 1.8528800 $F_{262°}$ = 71.265
Experiment 74.600
Dalton's table 69.700
Pouillet's table 70.800
Betancourt 82.500

The disparity between Biot's formula and experiment becomes more apparent now: it amounts to 3.385 inches.

At 266° FAHR. which corresponds to 130° centigrade, I make it from BIOT's first formula 77.053, while at 130° by M. POUILLET, it is 75.68;* difference 1.973. Finally,

At the temperature of 312°, $f = 100$

$100\ a =$		0.854121972
$10.000\ b =$	$-$	0.208109100
$1000000\ c =$	$+$	0.005800000
		0.651812872
		1.477121300
Log. of $F_f =$		2.128934172

$F_f = F_{100} =$ 134.57

Experiment gives 167.00

Mr. DALTON's table 125.85

The difference between experiment, and both calculations, is now excessive, and even between the two latter it amounts to nearly 9 inches.

From this ample investigation, we may legitimately conclude, that we ought to receive such geometrical representations with great caution. M. BIOT, indeed, with a candour becoming his genius, admits these formulæ to be merely tentative approximations. The high reputation of this philosopher, and the geometrical skill here displayed, might have led the scientific world to repose confidence in his formula, within the limits of $55\frac{1}{2}$ degrees centigrade = 100 FAHR. It was therefore entitled to a deliberate examination.

It is curious to observe that my very simple formula, Log. F = Log. 28.9 $\pm$ *n*. Log. *r*, gives good approximations,

* 130° centigr. gives by M. P. force of vapour = 1907.07 millemetres, of which taking 25.4 to the English inch, we have $\frac{1907.07}{25.4} = 75.08$ as above.

through a much more extensive range, than the elaborate formula of the distinguished French geometer. Even when carried so high as the 310th degree of FAHR., we have

$$\text{Log. } 28.9 + n.\ \log. r = 2.19810 = \text{L}, \text{F}_{100}; \text{ hence}$$
$$\text{F}_{100} = 157.8$$

Experiment gives 161.3, a difference of only $3\frac{1}{2}$ inches at this prodigious elasticity; which may be deemed altogether unimportant in practice.

BIOT's formula gives a result 31 inches, and Mr. DALTON's 40 in defect.

Of Professor ROBISON's higher numbers, it is merely necessary to examine the successive differences for every 10° above 212°. These are 7.2, 8.9, 10.2, 11.9, 13.5, 13.8, 11.8, and the second differences are + 1.7 + 1.3 + 1.7 + 1.6 + 0.3 − 2.0.

Such striking irregularities cannot exist in the progression of nature. BETANCOURT's are liable to a similar censure. We may find indeed small discrepancies in the best observations at such temperatures.

§ II. *Experiments to determine the elastic forces of the vapours of alcohol, ether, oil of turpentine, and petroleum or naphtha.*

The determination of the elasticities of these vapours is a very interesting problem in chemical philosophy. It may possibly unfold the law which connects temperature and elastic energy, and it may furnish likewise some useful applications.

Mr. DALTON has examined the subject with considerable care.

My experiments were performed with the apparatus above described, and were verified by frequent repetitions. The following results were noted down during the progress of the experiments.

Table III.

Elastic forces of the vapours of alcohol, ether, oil of turpentine, and petroleum or naphtha.

Ether.		Alcohol sp. gr. 0.813.		Alcohol sp. gr. 0.813.		Petroleum.	
Temp.	Force of Vapour.	Temp.	Force of Vapour.	Temp.	Force of Vapour.	Temp	Force of Vapour.
34°	6 20	32	0.40	193°.3	46.60	316°	30.00
44	8.10	40	0.56	196.3	50.10	320	31.70
54	10 30	45	0 70	200	53.00	325	34.00
64	13.00	50	0.86	206	60.10	330	36.40
74	16.10	55	1.00	210	65.00	335	38 90
84	20.00	60	1.23	214	69.30	340	41.60
94	24.70	65	1.49	216	72.20	345	44.10
104	30.00	70	1.76	220	78.50	350	46.86
		75	2.10	225	87.50	355	50.20
2nd.	Ether.	80	2.45	230	94.10	360	53.30
		85	2.93	232	97.10	365	56.90
105°	30.00	90	3.40	236	103.60	370	60.70
110	32.54	95	3.90	238	106.90	372	61.90
115	35.90	100	4.50	240	111.24	375	64.00
120	39.47	105	5.20	244	118.20		
125	43.24	110	6.00	247	122.10	Oil of Turpen.	
130	47.14	115	7.10	248	126.10		
135	51.90	120	8.10	249.7	131.40	Temp.	Force of Vapour.
140	56.90	125	9.25	250	132.30		
145	62.10	130	10.60	252	138.60		
150	67.60	135	12.15	254.3	143.70	304°	30.00
155	73.60	140	13 90	258 6	151.60	307.6	32.60
160	80.30	145	15.95	260	155.20	310	33.50
165	86.40	150	18.00	262	161.40	315	35.20
170	92.80	155	20.30	264	166.10	320	37 06
175	99.10	160	22.60			322	37.80
180	108.30	165	25 40			326	40.20
185	116.10	170	28.30			330	42.10
190	124.80	173	30.00			336	45 00
195	133.70	178.3	33.50			340	47.30
200	142.80	180	34.73			343	49.40
205	151.30	182.3	36 40			347	51.70
210	166.00	185.3	39.90			350	53.80
		190	43.20			354	56.60
						357	58.70
						360	60.80
						362	62.40

Remarks on the preceding table.

The ether of the shops as prepared by the eminent London apothecaries, boils generally at 112°; but when washed with water, or re-distilled, it boils at 104° or 105°. It may by rectification, however, be made to boil at a still lower temperature.

Concerning the boiling point of oil of turpentine, curious (may we say ridiculous) discrepancies exist in our systems of chemistry. Dr. MURRAY, for example, in the table of the scale of temperature at the end of the first volume of his valuable system, last edition, places the boiling point of oil of turpentine at 560°. Mr. DALTON, vol. 1. p. 39. of his new system of chemical philosophy, says "several authors have it that oil of turpentine boils at 560°. I do not know how the mistake originated, but it boils below 212°, like the rest of the essential oils." I made with much care several experiments on this point, previous to ascertaining the force of its vapour, and found its boiling point to be about 316°. When recently distilled, however, it will boil at 305°. Did it boil below, or even at 212°, as Mr. DALTON asserts, *then*, long before the included portion in the above experiments had reached the 304th degree, it would have acquired such an elasticity as to support a high column of mercury, instead of being barely *in equilibrio* with the atmospheric pressure.

Plunge a phial half filled with fresh oil of turpentine into a metal cup containing any fixed oil. Heat the cup gradually. It will be found that, at the temperature of 316°, the oil remains in steady ebullition, as indicated by a thermometer suspended in the centre of the phial. Prior to this, even at

212°, some small bubbles will be evolved, principally owing to the moisture dispersed in the pores of the oil, from the water originally mixed with the crude turpentine in its distillation. If the heat be very rapidly thrown in, while the upper surface of the oil of turpentine has the area only of a one or two ounce phial, it is possible to heat it to 360° or 370°, in apparent contradiction to the theory of latent heat; for when a liquid boils in an open vessel, according to Dr. BLACK, its temperature should remain stationary. The true cause of this phenomenon is developed towards the conclusion of this memoir. The specific caloric of the vapour of the volatile oil is so small, compared to that of water, that the heat may readily be quicker introduced than the boiling process can abstract it. Concerning the boiling point of this oil, I have since inquired of a manufacturer; and he states its boiling point at 320°. Essential oil of rosemary, when kept for some time, boils at 270°; recent oil at 212°. To assign the cause of this difference, is foreign to our present object.

The vapour of ether follows nearly the same rate of expansion as water, if we start from their respective boiling points. This was observed also in Mr. DALTON's experiments; and from this single analogy, chiefly, he laid down the general law, "that the variation in the force of vapour from all liquids "is the same for the same variation of temperature, reckon-"ing from vapour of any given force."

My experiments on oil of turpentine and petroleum show the fallacy of this generalization, if we reckon the common thermometric scale a tolerably correct index of temperature; but if, with Mr. DALTON, we consider our thermometric scale,

as very erroneous, then ether itself is an exception to its own law, to use this paradoxical, though just expression. In consequence of his peculiar thermometric ideas, Mr. DALTON has abrogated the above law, which he had himself framed; though it is curious to observe, in some respectable treatises on chemistry, both hypotheses detailed, without indicating their mutual incompatibility. M. BIOT, likewise, far from imagining that the law had been repealed for 8 or 9 years, proposes to judge by its provisions of the total elastic force of every vapour at 100° centigrade, to serve as the basis of the determination of their respective specific gravities at that temperature.*

My experiments show that from 105° to 167°.5 FAHRENHEIT, ether trebles the tension of its vapour, as water also does from 212° to 272°.7; both containing nearly, but by no means exactly, equal intervals of the FAHRENHEIT graduation. According to Mr. DALTON's corrected scale of temperature, we have,

212° FAH.	= 212° DALTON.	105° FAH.	= 119° DALTON.
272 F.	= 256.4 D.	167.5 F.	= 176 D.
real interval =	44.4 by DALTON.	By DALTON	57 = the real interval of temperature.

Thus we see, that while the interval for trebling the tension of ethereal vapour is 57°, that for aqueous vapour is only 44°.4; quantities that are to each other nearly as 100: 80. Hence, according to this eminent chemist, ether must take for

* "On peut calculer par la loi de M. DALTON, quelle doit être, pour chacun d'eux, la force élastique totale de sa vapeur à la température de 100 degrés." Traité de Physique, Tome i. p. 393.

trebling the force of its vapour a fifth part more heat than water does.

I hope presently to be able to adduce satisfactory experimental evidence, that our thermometric indications are not at all so unequable as Mr. DALTON conceives.

Meanwhile, in examining closely the table of the vapour of ether, a beautiful analogy with that of water presented itself. The series of ratios representing the progression of the latter being lowered a single step, will accurately fit the former. At 30 inches of elasticity 1.23 was our initial number for aqueous vapour; for ethereal, it becomes 1.22; increasing or diminishing by unity each time in the second decimal figure, according as we descend or ascend by intervals of 10° of the FAHRENHEIT scale.

The following is a general view of the results.

TABLE IV.

The observed tension of ethereal vapour compared with the ratios 1.22, 1.23, *&c. and* 1.22, 1.21, *&c.*

Temp.	Quotients.	Expert.	Temp.	Product.	Expert.
104°	—	30.00	105°	—	30.0
94	24.7	24.70	115	36.6	35.9
84	20.2	20.00	125	44.3	43.24
74	16.3	16.10	135	53.4	51.9
64	13.06	13.00	145	63.6	62.1
54	10.3	10.3	155	75.4	73.6
44	8.1	8.1	165	88.2	86.4
34	6.35	6.2	175	102.0	99.1
			185	117.3	116.1
			195	134.0	133.7
			205	151.3	151.3

The numbers derived from calculation give a surprising accordance with those observed in the lower range. In the

upper range, the correspondence is as good as the delicacy of the experiments at such temperatures could permit us to expect. The experiments have been presented without modification. I must own, that when first the above perfect coincidence appeared, it gave me no small pleasure, as it led me to suppose that I had discovered the hidden chain of nature.

In treating of the vapour of alcohol, Mr. Dalton considers it as irregular in the progress of its elastic force by heat, owing to its not being a homogeneous liquid. He suspects "that the elastic force in this case is a mixture of aqueous and alcoholic vapour." I cannot see the cogency of this argument; for, if the separate bodies have a regular progression, the mixture ought not surely to be anomalous. I believe, however, that if the experiments were made with due accuracy, alcohol would be found as methodical in the elastic march of its vapour as other bodies. The following table will afford satisfactory proofs of the justness of these views. For absolute alcohol, the progression is probably as simple as that of the preceding vapours. But for alcohol, sp. gr. 0.813, which though highly rectified, contains not a little water, we should expect it to result from a composition or modification of ratios. After some search on this principle, I accordingly found it. Starting from the boiling point 174°, or for the convenience of comparison with the table, from the decade 170°, we move not by a unit, as before, but by a unit and a tenth; or the initial ratio 1.26 is affected at each step or term of 10°, with the number $\pm$ 0.011, the signs being employed as in the preceding cases.

TABLE V.

Elastic force of the vapour of alcohol compared with the ratios.

Temp.	Calculat.	Observed.	Temp.	Calculat.	Observed.	Temp.	Calculat.	Observed.
250°	130.24	132.3	170°	28.3	28.3	90°	3.41	3.4°
240	111.13	111.24	160	22.46	22.6	80	2.52	2.45
230	93.94	94.1	150	17.7	18.6	70	1.85	1.76
220	78.67	78.5	140	13.8	13.9	60	1.35	1.23
210	65.29	65.0	130	10.65	10.6	50	0.97	0.86
200	53.69	53.0	120	8.16	8.10	40	0.69	0.56
190	43.76	43.2	110	6.2	6.00	30	0.49	0.38
180	35.35	34.73	100	4.67	4.50			

$\frac{28.3}{1.26} = 22.46$ ∴ $28.3 \times \frac{}{1.26 - .011} = 35.35$

$28.3 \times \frac{}{1.26 - .011 \times 1.26 - .022} = 48.76$ &c.

$\frac{22.46}{1.271} = 17.7$, &c.

The correspondence here exhibited between the observed and calculated elasticities is remarkable; nor does the difference ever exceed what would be produced by an error of 1° in the construction or reading off of the thermometer. This may fairly be deemed the limit of accuracy in such an experiment.

Oil of turpentine is regulated by the constant ratio 1.122, which converts any elastic force into that 10° above or below, multiplying as usual in the former, and dividing in the latter case. For petroleum the ratio is 1.14; it is also constant.

The following table exhibits a comparative view of theory and experiment.

TABLE VI.

Oil of Turpentine.			Petroleum.		
Temp.	Calculat.	Observed.	Temp.	Calculat.	Observed.
310°		33.5	320°		31.7
320	37.7	37.66	330	36.2	36.4
330	42.5	42.1	340	41.2	41.6
340	47.7	47.3	350	47.0	46.86
350	53.5	53.8	360	53.6	53.3
360	60.4	60.8	370	61.1	60.7

The whole of the preceding research is closely interwoven with a question of the first importance in chemical philosophy; what are the relative portions of temperature denoted by the graduations of our thermometric scale? Mr. DALTON regards the progressive elasticities of aqueous and ethereal vapour as affording countenance, if not support, to his thermometric innovations. He affirms, that if our instrument for measuring heat were accommodated to his doctrine, the quantity of expansion of its mercury is as the square of the temperature from its freezing point; then "the force of "steam in contact with water increases *accurately* in geome-"trical progression to equal increments of temperature, "provided these increments are measured by a thermometer "of water or mercury, the scales of which are divided by "the above mentioned law."*

Were this position true, it would certainly bring a powerful analogy in aid of his theoretical views. We are now furnished with *data* to verify, or refute it. The following tables show the correspondence between that principle and experiment. In the table of aqueous vapour, the *first* column pre-

* New System, vol. i. p. 11.

sents his geometrical progression of that vapour, co-ordinate with his equal intervals of real temperature contained in the *second*. In the *third*, are the corresponding points of the common scale, as given by Mr. DALTON. To these points the elastic forces, as determined by experiment, are placed opposite in the fourth column.

Table second, for vapour of ether, is similarly arranged; the first three columns being Mr. DALTON's; the *fourth*, the faithful transcript of observation.

"The force of the vapour of sulphuric ether," says Mr. DALTON, "in contact with liquid ether, is a geometrical pro-"gression, having a less ratio than that of water." "Ether, "as manufactured in the large way, appears to be a very "homogeneous liquid. I have purchased it in London, Edin-"burgh, Glasgow, and Manchester, at different times, of "precisely the same quality in respect to its vapour."* This shows that no exception can be made to my experiments on account of a supposed difference in the quality of the ether. From the mode of conducting my experiments, there remained always a quantity of liquid ether in contact with the vapour, a circumstance essential to accuracy in this research. The results were verified by frequent repetitions, and discover, in my opinion, the consistency of truth.

* New System, vol. i. pp, 20, 21.

TABLES VII. AND VIII.

DALTON's *theory of the thermometric scale, compared with the observed temperatures and tensions of vapours.*

Aqueous Vapour.				Ethereal Vapour.			
DALTON's geom. progression of elasticity.	DALTON's new scale of temperat.	FAHREN.	Observed elasticity.	DALTON's progression of elasticity.	DALTON's scale.	FAHREN.	Observed elasticity.
22.7 inch.	202°	199°	23.1 in.	6.1	32°	32°	5.81
30.0	212	212	30.0	9.16	52	46.6	8.67
39.5	222	225	39.11	13.77	72	62.55	12.60
52.0	232	238.6	50.3	20.65	92	79.84	18.40
69.0	242	252.6	64.5	31.0	112	98.50	27.2
91.0	252	266.8	81.5	46.54	132	118.50	37.7
120.0	262	281.2	103.5	69.88	152	139.9	56.8
158.	272	296.2	131.7	104.91	172	162.4	83.3
208.	282	311.5	164.8	157.5	192	186.5	118.3
				236.5	212	212	169.0

The numbers of the first and fourth column ought evidently to agree, if the theory be just. Their differences, on the contrary, are prodigiously great. At 272° of his scale, for example, equal to 296°.2 of ours, the law of progression makes the elastic force of aqueous vapour amount to 158 inches: experiment gives 131.7; and I am confident, that the latter cannot be in error above an inch or two. Again at 262°, equivalent to 281°.2 FAHRENHEIT, his theory gives the force of the same vapour at 120 inches; by observation it is only 103.5. Now at this part of the scale, my result is confirmed by the concurrence of those obtained by BETANCOURT and ROBISON. I consider this demonstration complete. If we compare these very elasticities of Mr. DALTON, with the table formerly given by the same philosopher,* we shall find

* Manchester Memoirs, vol. 5.

discordances which no ingenuity can harmonize. At that time, 225° of FAHR. = 222° of the new scale, gave a force of vapour equal to 38.3; it is now 39.5. 252°.6 F. = 248° D. then coincided with an elasticity of 38.6 inches; above, it is 69. And finally, 281°.2 F. = 262° D. were opposite to 90 inches; they have become here 120. And yet no new experiments on the vapour of water have been adduced, to justify such immense alterations.

It may be said, indeed, that these changes arise merely from the substitution of one hypothesis for another; but the deviations from experiment are even more remarkable, since as 282° new scale, correspond to 311°.5 FAHR., the difference amounts to 48 inches, being more than one fourth of the total elastic force generated at that high temperature.

When we turn our attention to ether, we find the discrepancies, if possible, less easy to reconcile. At the temperature of 212°, for example, where the old and new scales meet for the last time, the force of its vapour by the geometrical progression exceeds that found from experiment, by the enormous quantity of 67 inches and a half; amounting to two fifths of the whole elastic force evolved.

May we venture, then, to conclude, from these multiplied comparisons, that the progressions of elasticity in vapours, taught by Mr. DALTON, are geometrical fictions, intended to quadrate with his notions concerning temperature; but not consonant with the laws or phenomena of nature?

Within a moderate compass, indeed, it is not difficult to suit the ratio of elastic force and the thermometric graduation to each other; but the prosecution of the enquiry into ranges more remote, detects the fallacy of such hypothetical adapta-

tions. My experiments on the vapours of water, alcohol and ether, seem to show, that the ratio of tension decreases in a certain progression as the temperature augments. Were the ratios 1.23, 1.22, 1.21, &c., which are seen to apply so well to aqueous vapour for a considerable range above 212°, to be adopted as representing the progressive march of nature, it would lead to the absurd conclusion, that at 240° above the boiling point, or 452° F., the farther influx of caloric would occasion a diminution of elasticity in the steam. The truth however is, that at the 31sth degree, indications of a divergence begin to appear between the two lines of experiment and calculation, which had run for so long a space nearly parallel. The curve representing the expansive force of steam, I consider to be logarithmic, in which the ratios, as ordinates, continually diminish, without ever vanishing, or coming to an equality. The axis is an asymptote to the curve, as in the atmospherical logarithmic.

CHAPTER II.

On thermometric admeasurement, and the doctrine of capacity.

Before inquiring into the relative quantities of heat, contained in different vapours at the same tension, it will be proper to determine the primary and fundamental proposition concerning the measure of temperature. It is singular, that not one experimental fact has been advanced, capable of settling this question, amid the contending opinions of chemical philosophers. Mr. DALTON has, in particular, exerted all the resources of his genius and science to destroy our confidence in the thermometric scale; our sole guide in the vast

and intricate province of caloric. While I hope to be able to fix this now indeterminate point, by a new train of investigations, and consequently to prove the entire fallacy of his doctrine of temperature, the key-stone of his system of heat, I do not mean to affirm the absolute uniformity of expansion in bodies, by equal increments of that power. I think it indeed highly probable, that every species of matter, both solid and liquid, follows an increasing rate in its enlargement by caloric. Each portion that enters into a body must weaken the antagonist force, cohesion; and must therefore render more efficacious the operation of the next portion that is introduced. Let 1000 represent the cohesive attraction at the commencement; then, after receiving one increment of caloric, it will become $1000 - 1 = 999$. Since the next unit of that divellent agent will have to combat only this diminished cohesive force, it will produce an effect greater than the first, in the proportion of 1000 to 999; and so on in continued progression. That the increasing ratio is, however, greatly less than Mr. DALTON maintains, may, I think, be clearly demonstrated.

According to his table of equal increments of temperature, vol. i. p. 14, New System, we have the following intervals, corresponding to the five successive intervals of 90° on our scale.

From 32° to 122°, to 212°, to 302°, to 392°, to 482°.
Intervals by FAHR. of 90°, 90°, 90°, 90°, 90°.
True intervals by DALTON, 102°.4 77°.6 63°.9 55°.7 50°.5

The relative inequality of these intervals is deduced from Mr. DALTON's law, that "all pure homogeneous liquids, as "water and mercury, expand from the point of their congela-

"tion, or greatest density, a quantity always as the square "of the temperature from that point." He regards the law as resulting from the constitution of liquids, and therefore not applicable to solid bodies. This is indeed implied in its enunciation. In p. 43, after assigning reasons, he states, "that for all practical purposes we may adopt the notion of "the equable expansion of solids."

Now I am prepared to prove, either, that the expansion of solids partakes of the above inequability of liquids, which nobody imagines, and for which no reason, even hypothetical, can be assigned; or, which is the only alternative, that homogeneous solids, and mercury, proceed almost exactly, *pari passu*, in their rates of expansion by heat.

The experiments which justify this assertion were made by me about five years ago, and were then exhibited to many of my chemical friends, as also in my public lectures; but a wish to render the series more complete, has induced me to withhold them from the public eye, till requisite leisure could be afforded for this purpose. They were performed with a pyrometer of peculiar construction, in an oblong trough filled with melting ice: a strong bar of Swedish iron was placed, from which projected at right angles, four inflexible iron arms, attachable by powerful screws to any part of the bar. The arms nearest the extremities of the bar, carried each a fine micrometer microscope, made by that admirable artist Mr. TROUGHTON. The other two arms were incurvated downwards at their extremities, which supported a metallic or other rod. This was fixed by two pinching screws at one end, but lay loose on a friction roller at the other. The loose end bore an elevated index. The curvature of these two arms was

such as to allow their extremities, with the attached rod, to be plunged *beneath the surface of oil or water, about an inch*, contained in a copper trough. This was placed parallel to the large trough, and a few inches distant from it.

The copper vessel was slowly and equably heated, by a series of argand lamps placed beneath. One micrometer watched a point projecting from the arm that held the fixed extremity of the rod. The oil was carefully agitated during the application of the heat; and the bulbs of three thermometers, mutually comparable, were immersed into it at regular distances. The micrometers were screened from the influence of the heat. They rendered the $\frac{1}{10000}$ of an inch discernible, and even a smaller quantity, by an experienced eye.

A rod of pure Swedish iron, or of such pure copper as jewellers use for alloying gold, being adjusted to the apparatus, the point on the micrometer scale, that appeared a tangent to the small luminous aperture in the thin index plate of steel, was noted down, when the liquid in the trough was at 32°. The value and truth of the micrometrical indications had been previously ascertained, by viewing through the microscopes a given surface or aperture, moved laterally, so as to make its image successively coincide with the different points of the interior notched scale.

Heat being now applied, the progressive march of the index across the field of view of the micrometer microscope was closely observed, and its position written down at intervals of 10° or 20° of the Fahr. thermometer. But as the pyrometrical details will appear in a separate memoir on the expansions of bodies, I shall state here merely what concerns the present subject.

If we denominate the absolute elongation of the heated metallic rod from 32° to 122°,10, then its elongation from 122° to 212°; from 212° to 302°; from 302° to 392°; from 392° to 482°, was in each successive interval of 90° F, as nearly as possible 10 also. The slight irregularities, incident to all delicate experimental investigations, being often in opposite directions, in different repetitions of the same experiment; or those which manifested themselves in the ascending or elongating range, were neutralized, so to speak, by others of an inverse nature, which appeared in the cooling retrocession. *Here*, the movements of the liquid mercury and of the solid rod by heat proceeded, *pari passu*, through a very great extent of temperature. Let us now recollect that these 5 increments, which on our thermometer are equivalent to $5 \times 90° = 450°$; and which altogether produce five times the elongation that the first interval occasions, constitute, on Mr. DALTON's scale, only 350°. If we call the first interval given by this philosopher 1.00, then the four succeeding intervals contain a range of temperature on his scheme, of only two and a half times the first; and therefore only two and a half times additional elongation should have been produced, instead of four times, as found by experiment. "Since for all practical purposes uniform increments of bulk, or expansions of *solids* by heat, correspond to uniform increments of this power;" then each of our old successive intervals of 90° may, for all practical purposes, be held to correspond to equal increments of temperature.

Mr. DALTON's intervals from 32° to 482° FAHR. are as before given, 102°.4; 77°.6; 63°.9; 55°.7; 50°.5 = 350°.1. Now, if we call the first quantity 1.00, it will produce on a

metallic rod a corresponding effect in expansion = 1.00. The next interval of Mr. DALTON's scale (equal always to 90° FAHR.) can produce only ¾ of the effect of the first, or as 75 to 100. The third, fourth, and fifth intervals will give the fractional expansions in reference to the first, of $\frac{63}{100}$, $\frac{44}{100}$, and about $\frac{40}{100}$, or merely a half.

No such diminution of effect was observed in the experiments; from 392° to 482° F., the rod elongated as much as from 32° 122°, or double the quantity compatible with the DALTONIAN hypothesis. Thus therefore we have a rigid, and I think unanswerable demonstration of the general correctness of the common scale of temperature, and of the extreme inaccuracy and inapplicability of Mr. DALTON's geometrical substitute. Should the preceding statement leave any doubt or obscurity concerning the legitimacy of the inference now drawn, I trust it will be entirely removed, when the details of the experiments are published, with drawings of the apparatus, in my treatise on pyrometry.

Yet though the mercury in the thermometer tube move, *pari passu*, with a metallic rod, deemed uniform in its expansion, it does not prove perfectly equal uniformity of expansion to belong to the mercury. It will seem, no doubt, a paradoxical assertion, that of two bodies marching together, hand in hand, one of them may have an equable pace, while that of the other is regularly, but very slowly accelerated. Yet I think the position just. It proceeds from a circumstance in the thermometer sufficiently obvious, but which seems to have escaped our *systematic* writers. I do not rest the proposition on any imperfection of workmanship, or supposed irregularity in the expansions of the glass

Let us take a thermometer, the calibre of whose stem is perfectly uniform, and whose scale is exactly divided. Let it have a range from zero to the 656th degree, at which mercury boils, by the accurate experiments of CREIGHTON. At 32°, let the mercury stand at the bottom of the ivory scale, where of course the graduations commence. The bare part of the instrument is consequently the plunging limit, in most chemical researches on the temperature of liquids. Immerse the bulb in common oil, or oil of vitriol heated to 212°; $\frac{1}{63}$ * part of the whole included mercury, will now ascend above that part of the stem plunged in the liquid. The part actually exposed to the heat, and by whose expansion the column on the scale is supported, is only $\frac{62}{63}$ of the initial mass. Augment the heat of the oil till the instrument indicate 392°; we know that there remains now, under the immediate influence of the heat, $\frac{61}{63}$ nearly of the original weight of mercury; and finally, at 572°, only about $\frac{60}{63}$ rest in the immersed part of the stem and bulb.

$\frac{1}{63}$ or $\frac{1}{21}$ parts may be considered as no longer subjected to the power of caloric. If the thermometer stem were recurved near the bulb, the mercury in the stem placed horizontally would be cold; and this proposition would be almost exactly true.

Now, since the calibre and divisions are uniform, the capacity of the tube from the point marked 212°, to that marked 392°; and again from this, to that opposite to 572°, is in each equal to its capacity from 32° to 212°. Hence these three equal capacities are filled by the expansions of the three unequal quantities of mercury 62, 61, 60. At the

* A minute fraction less; but we need not complicate the statement with it.

highest station, the column of quicksilver equal on the stem to $3 \times 180°$, is sustained by the expansion of 60 parts; at the middle point, $2 \times 180°$ is supported by that of 61; and at 212° there are 62 parts of mercury to sustain 180° in the tube. Or, to put it in another form, these three successive spaces on the scale are equal; the first portion of mercury is protruded into it by the expansion of 62 parts in the bulb; the second portion by the expansion of 61; and the third by that of 60.

Therefore, if these three thermometric intervals of 180°, each of which holds an equal measure of mercury, contain also equal increments of temperature, as denoted by the equal increments of a metallic rod; then, these three equal effects are produced from the unequal quantities of mercury 62, 61, 60. This liquid, then, must have an increasing rate of expansion, the inverse of these numbers, for every 180° of the scale, or $\frac{1}{62}, \frac{1}{61}, \frac{1}{60}$. That is to say, 60 parts at 572° do the same work by the same power of caloric, as 61 at 392°, and 62 at 212°.

I believe this to be the real nature of mercurial expansion, and the true condition of the thermometer; which is an equable measurer of heat, because the mercury possesses the above increasing rate of expansion. Were the mercury, on the contrary, absolutely uniform in its augmentations of volume by equal increments of heat, then for an instrument whose bulb alone in practice can be immersed, the three above ranges should have the corresponding parts of the scale shortened in the successive proportions of 62 to 63; 61 to 63; and 60 to 63; quantities taken together nearly equal to 9°, or $= 3 \times \frac{1}{62} \times 180 = \frac{540}{62}$.

Whatever reception these speculations may experience, they must not be confounded with the experiments on the expansions of metallic rods, and the corollaries, which have a distinct and independent existence.

§ II. *On the doctrines of capacity, as connected with the preceding investigation.*

Dr. CRAWFORD and DE LUC, tried to verify the justness of the thermometric indications, by mixing together water at 212° and 32°; when the former found 122°, and the latter 119°, to be the resulting temperature. DE LUC's number is 3° below the mean; Dr. CRAWFORD's is exact. This ingenious philosopher afterwards sought to confirm the evidence thus given to the accuracy of the scale, by other experiments, which were however of rather an equivocal import. Both of the above results have been condemned and rejected by Mr. DALTON: he states the true mean temperature to be not 122°, nor even 119°, but 110°. For this deviation, the reasons which he assigns appear, independently of all arguments derived from other quarters, to be in themselves inconclusive. He says, "the temperature of the above mixture ought to be found above the mean 122°." "Water of these two tempe-" ratures (32° and 212°) being mixed, loses about $\frac{1}{90}$ of its " bulk. This condensation of its volume* must expel a " quantity of heat, and raise the temperature above the " mean." p. 7. Again, p. 50, "that water *increases* in its " capacity for heat with the increase of temperature, I con-" sider demonstrable from the following arguments. 1st. A

* That condensation of volume in a liquid, is no proof of the expulsion of heat, is shown in my Essay on Sulphuric Acid.

"measure of water at any one temperature mixed with a "measure at any other temperature, the mixture is less than "two measures. Now, a condensation of volume is a certain "mark of diminution of capacity and increase of temperature,* "as in the mixture of sulphuric acid and water; or the effects "of mechanical pressure, as with elastic fluids. Second, "when the same body suddenly changes its capacity by a "change of form, it is always from a less to a greater as the "temperature ascends; for instance, ice, water and vapour. "Third, Dr. CRAWFORD acknowledges from his own ex- "perience, that dilute sulphuric acid, and most other liquids "he tried, he found to increase in their capacity for heat with "the increase of temperature. Admitting the force of these "arguments, it follows, that when water of 32° and 212° are "mixed, and give a temperature denoted by 119° of the "common thermometer, we must conclude that the true mean "temperature is somewhere below that degree. I have "already assigned the reason why I place the mean at 110." Now the only reason I can elsewhere find, is derived from his general law, "that all homogeneous liquids expand, as the square of the temperature, from the point of greatest density or congelation." In p. 7, he ventures to assert nothing more than, "that it is not improbable that the true mean tempe- "rature between 32° and 212°, *may* be as low as 110° FAH- "RENHEIT."

Satisfied from my pyrometrical experiments, that his general hypothesis of the expansion of liquids being as the

* For the entire fallacy of this reasoning, see my Essay just quoted; expansion of volume should by Mr. D. increase capacity and diminish temperature. The very reverse is shown in that paper.

square of their temperature, is totally inapplicable to mercury, the inference relative to the thermometric mean between 32° and 212° cannot be allowed. But let us examine, on their own merits, the preceding arguments against Dr. CRAWFORD and DE LUC's verification of the mean temperature between that of freezing and boiling water.

The reasoning derives its sole force from the assumption, that the capacity of water for heat, increases as its temperature is raised. There is adduced, however, no fact in the least decisive on this main point. What analogy is there between the entire change of form and constitution suffered by an incondensible liquid, on becoming an elastic vapour, and the progressive heating of the liquid itself? Or, although dilute sulphuric acid and other liquids should increase in their specific caloric on being heated, which however has not been satisfactorily demonstrated, are we to assert that water must do so too? It is a matter of surprize to me, that a philosopher of Mr. DALTON's judgement and acuteness should have pressed such inconclusive analogies into his service. He knew well that water is endowed with some curious peculiarities, when compared with other liquids, or anomalies, as we idly stile them; for they constitute no anomaly in nature, but wisely fit water for performing the important functions assigned to it in the economy of our globe.

In a series of experiments, carefully conducted on the relative capacities for heat, of water, sulphuric acid, oil of turpentine, and spermaceti oil, published in my Essay on hydrochloric acid and the chlorides; it seems to be directly demonstrated that the specific heat of water does not *increase*, but actually *diminishes*, and that very conspicuously, as its

temperature rises. It is there proved, that from 210° to 150° FAHR. the specific heat of oil is to that of water as 697 to 1000; and from 150° to 90° as 513 to 1000. The same proportional difference of relation is exhibited by the other two liquids. Now, were the phenomenon occasioned by the oil of vitriol, common oil, and oil of turpentine, increasing in *their* capacities for heat in a still more rapid ratio than water, we should undoubtedly expect, from the innate differences between the specific heats of these three substances, to find that they would move independently on each other, or at different rates. But *their* uniform advance together, while water alone varies in this respect, shows distinctly, that in the water resides the cause of the variation. This reasoning may be illustrated in many ways, but by nothing more clearly than the exploded astronomical system of the diurnal and annual movements of the sun and fixed stars; in support of which, very extravagant hypotheses had to be contrived.

The single fact of the motion of the earth once admitted, reduced the PTOLEMAIC chaos to order. If, in like manner, we should suppose an increasing ratio in the specific heat of water, then we must also suppose a much more rapid increase in the ratios of the above three substances, although their individual specific heats are greatly inferior to that of water. Ought not that body, which has of all others the most decided relation to heat, or highest specific heat, to have also its ratio most decidedly or rapidly augmented? In adopting the increasing specific heat of water, we must farther assume, that, however different the initial specific heats of the above three liquids may be, yet, while they possess all the same rate of increase, water alone has a different one; an inadmissible supposition. All

these difficulties and contradictions are removed at once by the experimental fact, that water is endowed with a decreasing ratio in its capacity for caloric, as its temperature is augmented.

Since finishing the above researches on specific heat, I have been led to examine attentively the systematic accounts of this subject in our chemical treatises; and I find that BERTHOLLET, with a sagacity peculiar to himself, had anticipated, from the chemical constitution of bodies, such an experimental result as I have recently obtained; though the statements then prevalent all militated against his views. "If caloric "obey the usual laws of attraction, when it is in small "quantity, relative to the body to which it is united, it will "enter into more intimate combination; and hence the elas- "ticity or expansive energy of it, on which temperature "depends, may be overcome, and a larger quantity be required "to produce a given temperature. Hence, the quantity of "caloric contained in bodies in the first stage of temperature, "may be greater than it will be higher in the scale."

In the Essay above referred to, I have shown that this circumstance in water, renders it peculiarly qualified for serving as the magazine and equalizer of the temperature of the globe. Since at our ordinary atmospherical heats, it possesses the greatest capacity for caloric, small variations in its temperature give it a great modifying power over the circumambient air. Although the doctrine of final causes be no safe guide to the discovery of unknown truths, yet when it concurs with experiment, we may deem it an agreeable confirmation. This is finely illustrated by Count RUMFORD's speculations on the maximum density of water being placed

several degrees above its point of congelation; a fact which does not hold with regard to any other homogeneous liquid.

If the specific heat of water, then, diminish as its temperature advances from the freezing to the boiling point, an interval of 10° near 32°, will contain more caloric than ten degrees near 122°, and still more than the same intervals near 212°. On this principle we can readily account for the results obtained by Mr. DALTON, in mixing with water at different temperatures a known proportion of ice; though it is remarkable that this able chemist did not see in them any thing inconsistent with his own opposite views upon specific heat.

"176°.5 expresses the number of degrees of temperature, "such as are found between 200° and 212° of the old or com-"mon scale, entering into ice of 32°, to convert it into water of 32°; 130° of the same scale suffice, he says, for the same effect, between 122° and 130°; and between 45° and 50°, 128° are "adequate to the conversion of the same ice into water. "These three resulting numbers (128, 150, 176.5) are nearly "as 5, 6, 7. Hence it follows, that as much heat is necessary "to raise water 5° in the lower part of the old scale, as is "required to raise it 7° in the higher, and 6° in the middle."*

Mr. DALTON, instead of adopting the obvious conclusion, that the capacity of water for heat is greater at lower than it is at higher temperatures, and that therefore a smaller number of degrees of the former should melt as much ice as a greater number of the latter, ascribes the deviation denoted by these numbers, or their differences, to the gross errors of our thermometric graduation; which he considers

* New System, vol. i. p. 53.

so excessive, as not only to equal, but greatly to overbalance the real increase in the specific heat of water; which left to its own operation, would have produced opposite experimental results.

That our thermometric scale has no such prodigious deviation from truth, or uniformity of indication, I conceive to be fully established, and therefore the only legitimate inference from these very experiments of Mr. DALTON, is the *decreasing* capacity of water with the increase of its temperature.

It deserves to be remarked, that my experiments on the relative times of cooling a globe of glass, successively filled with water, oil of vitriol, common oil, &c. give exactly the same results as Mr. DALTON derived from mixtures of 5 ounces of ice and 60 of water. This concurrence is the more satisfactory, since, when the Essay on hydrochloric acid was written, I had no recollection of Mr. DALTON's experiments. I found that from 210° to 150° the specific heat of oil bears to that of water the ratio of 597 to 1000; and from 150° to 90°, that of 513 to 1000. Now, at his highest and middle temperatures of 200° and 120°, which come nearest to mine of 180° and 120°, we have by him the ratio of 176°.5 to 150°.

But 597 : 513 :: 176 : 150 exactly, which is a very striking coincidence, and affords the happiest confirmation of the accuracy of both sets of experiments, as well as of the justness of the principles on which they were conducted, and on which, particularly, my reductions were founded. We now see the reason why, when equal weights of water at 32° and 212° are mixed, the temperature may be *below* the mean, as was found by DE LUC. The capacity at the middle tempe-

rature, is greater than the mean capacity of the two extremes, (that is of the ingredients mixed, ice, cold and boiling water) and therefore the thermometric tension will be lessened, and its mercury will descend on the scale.* This diminution of temperature will cause a corresponding diminution of bulk, which affords a complete answer to Mr. DALTON's first and only plausible argument, formerly quoted against Dr. CRAWFORD's deductions, and the opinions of DE LUC. With regard, however, to these experiments, of mixing hot and cold water to find a mean temperature, there are sufficient difficulties to render the result uncertain to 2 or 3 degrees. Hence, nothing of moment can safely be inferred from them.

Concerning sulphuric acid in its various states of dilution, I beg to refer the reader to my Essay on the subject, where he will find several peculiarities relative to its volume at different acid strengths, that entirely change its relations to caloric. I have not seen these formerly adverted to by any chemist. They were evidently unknown to Mr. DALTON.

CHAPTER III.

On the latent heat of different vapours.

What relation is there between the caloric existing in the vapours of different substances, and the temperatures at which they respectively acquire the same elastic force?

On this subject I am not acquainted with any preceding inquiries, though a question of such interest has probably not escaped examination.

* Taking Mr. DALTON's three numbers as correct: then $\frac{176.5 + 128}{2} = 152°.25$. But 150° in the middle are equal to the former mean of the two. Hence, the proposition in the text is demonstrated.

In this research I employed a very simple apparatus; and with proper management, I believe it capable of giving the absolute quantities of latent heat in different vapours, as exactly as more refined and complicated mechanisms. At any rate, it will afford comparative results with great precision.

It consisted of a glass retort of very small dimensions, with a short neck inserted into a globular receiver of very thin glass, and about three inches in diameter. The globe was surrounded with a certain quantity of water at a known temperature, contained in a glass basin. 200 grains of the liquid, whose vapour was to be examined, were introduced into the retort, and rapidly distilled into the globe by the heat of an argand lamp. The temperature of the air was 45°, that of the water in the basin from 42° to 48°, and the rise of temperature, occasioned by the condensation of the vapour, never exceeded that of the atmosphere by 4 degrees. By these means, as the communication of heat is very slow between bodies which differ little in temperature, I found, that the air could exercise no perceptible influence on the water in the basin during the experiment, which was always completed in 5 or 6 minutes. A thermometer of great delicacy was continually moved through the water; and its indications were read off, by the aid of a lens, to small fractions of a degree.

In all the early experiments of Dr. BLACK on the latent heat of common steam, the neglect of the above precautions introduced material errors into the estimate. Hence that distinguished philosopher found the latent heat of steam to be no more than 800° or 810°. Mr. WATT afterwards deter-

mined it more nearly from 900° to 950°; and LAVOISIER and LA PLACE have made it 1000°.

It is evident that whenever the water, into which the latent heat is evolved by condensation of the vapour, becomes much hotter than the surrounding air, it will be impossible to ascertain how much of the caloric is dissipated; and consequently, the true quantity contained in the vapour must remain uncertain.

The sources of error in operating with the calorimeter of LAVOISIER and LA PLACE, were first pointed out by Mr. WEDGWOOD, and have been since commented on by Dr. THOMSON, and other good systematists. It is said to be difficult to obtain precisely uniform quantities of liquified ice in the repetition of the same experiments, with that celebrated apparatus.

From the smallness of the retort in my mode of proceeding, the shortness of the neck, and its thorough insertion into the globe, we prevent condensation by the air *in transitu*; while the surface of the globe and the mass of water being great, relative to the quantity of vapour employed, the heat is entirely transferred to the refrigeratory, where it is allowed to remain, without apparent diminution, for a few minutes. In numerous repetitions of the same experiment, the accordances were excellent. The following table contains the mean results. The water in the basin weighed in each case 32340 gr. The globe was held steadily in the centre of the water by a slender ring fixed round its neck. The distillation was rapidly performed, so that all the condensation took place in the globe.

Table of experimental results on the latent heat of different vapours.

200 gr. of water distilled, raised	32340 gr. water from	42°.5 to 49°
200 gr. alcohol, spec. gravity 0.825	-	42 to 45
200 gr. sulphuric ether; boiling point 112°		42 to 44
200 gr. oil of turpentine	- -	42 to 43.5
200 gr. petroleum	- - -	42.5 to 44
200 gr. nitric acid. spec. grav. 1.494. boiling point 165°	- - -	42 to 45.5
200 gr. liquid ammonia. sp. gr. 0.978	-	42 to 47.5
200 gr. vinegar. sp. gr. 1.007	-	42.5 to 48.5

Calculation from the above table of the specific or latent heats of the vapours.

1st, Water. $\frac{32340}{200} = 161.7$ = the number of grains of water contained in the refrigeratory to one of steam. This proportion is constant for all the vapours.

From 42°.5 to 212° there are 169.°5; one half of which = 84°.75, or in round numbers 84°, is the rise of temperature which would be produced by adding to water at 42°.5 its own weight of boiling water; and $\frac{84}{161.7} = 0.52$, is the elevation which 200 gr. would occasion on 32340 grains.

The water was however in reality heated $6\frac{1}{2}$ degrees, or from 42°.5 to 49°. The difference, $6°.5 - 0°.52 = 5°.98$, shows the quantity of heat added to each of the 161.7 parts beyond what the same weight of boiling water would have communicated.

And $5.98 \times 161.7 = 967°$, being the latent heat of the steam of water.

2d. Alcohol. Boiling point 175°. Specific gravity 0.825.

$\frac{175 - 42 \times 66°.5}{2 \times 161.7} = 0.41$. 0.41 multiplied into the specific heat of liquid alcohol 0.65, is 0°.266, which represents the elevation of temperature produced by adding 200 gr. of boiling hot alcohol to 32340 gr. of water. The thermometer in the experiment rose 3°. $3° - 0°.266 = 2.734$, $2.734 \times 161.7 = 442°$ = the latent heat of alcoholic vapour in equilibrio with the atmospheric pressure. By a similar process of calculation the latent heat of the other vapours was determined.

General table of latent heat of vapours.

Vapour of water at its boiling point	967°
alcohol - -	442
ether - -	802.379
petroleum - -	177.87
oil of turpentine -	177.87
nitric acid -	531.99
liquid ammonia -	837.28
vinegar -	875.00

From the phenomena exhibited in the mechanical condensation and rarefaction of gases and vapours, as well as from their general constitution, it may be inferred, that an intimate and necessary connection subsists between their latent heat, elastic force, and specific weight or density.

Hence, when their tension is the same, it appears reasonable to suppose that the product of their densities into their quantities of latent heat will also be the same. Repulsive energy

will be proportional to the quantity of heat, the repulsive power condensed or contained in a given space. Thus if the *space* left for its interposition or lodgment be in one vapour a half or a third of the amount of the space in another, we ought to find equal tension produced in the former case, by a half or a third of the latent heat required for the latter.

As the principle, I have reason to suppose, is somewhat new, let us illustrate it by an application to the three vapours in the above list which are most homogeneous, or at any rate best understood; those of alcohol, ether, and water.

Aqueous vapour of an elastic force balancing the atmospheric pressure has a specific gravity, compared to air, by the accurate experiments of GAY LUSSAC, of 10 to 16.

For facility of comparison let us call the steam of water unity, or 1.00; then the specific gravity of the vapour of pure ether is 4.00, while the specific gravity of the vapour of absolute alcohol is 2.60.

But the vapour of ether, whose boiling point is not 100°, but 112°, like the above ether, contains some alcohol; hence, we must accordingly diminish a little the specific gravity of its vapour.

It will then become instead of 4.00 - -	3.55
Alcohol of 0.825 sp. gr. contains much water; specific gravity of its vapour - -	2.30
That of water as before, unity - - -	1.00

The interstitial spaces in these three vapours will therefore be inversely as these numbers, or

$\frac{1}{355}$ for ether; $\frac{1}{230}$ for alcohol; $\frac{1}{100}$ for water.

Hence $\frac{1}{355}$ of latent heat, existing in ethereal vapour, will occupy a proportional space, be equally condensed or possess the same tension with $\frac{1}{230}$ in alcoholic, and $\frac{1}{100}$ in aqueous vapour.

A small modification will no doubt be introduced by the difference of the thermometric tensions, or sensible heats, under the same elastic force. Common steam, for example, may be considered as deriving its total elastic energy from the latent heat multiplied into the specific gravity + the thermometric tension.

Hence the elastic force of water, or

$$E_w = 970^\circ \times 1.00 + 212^\circ = 1182$$

$$E_e = 302^\circ \times 3.55 + 112^\circ = 1184$$

$$E_{al} = 440^\circ \times 2.30 + 175^\circ = 1185$$

Three equations which yield, according to my general proposition, equal quantities; or of which the differences are inconsiderable, and undeserving of notice.

Neither the specific heats nor specific gravities of the other vapours are ascertained with sufficient precision to enable us to subject them to calculation.

General equation $F - \overline{L \times D} + T = 0$: L, latent heat, D, density, T, temperature corresponding to F.

When the elastic forces of vapours are doubled, or when they sustain a double pressure, their interstices are proportionally diminished. We may consider them now as in the condition of vapours possessed of greater specific gravities. Hence, the second portion of heat introduced to give double the

elastic force need not be equal to the first, in order to produce the double tension. This view now given accords with the experiments of Mr. WATT, alluded to in the beginning of this memoir. He found that "the latent heat of steam is less when it is produced under a greater pressure or in a more dense state; and greater when it is produced under a less pressure or in a less dense state."*

BERTHOLLET thinks this fact so unaccountable, that he has been willing to discard it altogether. Whether the view which I have just opened, of the relation subsisting between the elastic force, density, and latent heat of different vapours, harmonize with chemical phenomena in general, I leave to others to determine. It certainly agrees with that *unaccountable* fact. Whatever be the fate of the investigation of the general law now respectfully offered, the statement of Mr. WATT may be implicitly received under the sanction of his acknowledged sagacity and candour.

CONCLUSION.

To the theory of latent heat, which, like the hydrostatic paradox of Archimedes, might have remained for ages a barren, though beautiful proposition, the fertile genius of that philosopher gave all at once its noblest application, and most beneficial influence on human life, by his new steam engine. After him, many minds of the first order for science and ingenuity have offered schemes of farther improvement;

* Philos. Trans. vol. 84. p. 335.

but all either frivolous or abortive; with such prophetic judgment had Mr. WATT anticipated the happiest form and structure of which it was susceptible.

Under this conviction, it is with much deference that I draw the following practical inferences from the last train of experiments.

Since the vapour of alcohol, having the same elastic force as the atmosphere, contains $\frac{44}{100}$ of the latent heat of ordinary steam, and since its elastic force is doubled at the 206th degree (6° below the boiling heat of water), with perhaps $\frac{1}{3}$ of additional caloric; might we not, in particular circumstances, employ this vapour for impelling the piston of a steam engine? The condensing apparatus could, I imagine, be so constructed, as to prevent any material loss of the liquid, while more than a quadruple power would be obtained from the same size of cylinder at 212°, with an expenditure of fuel not amounting to one half of what aqueous vapour consumes; or the power and fuel would be as 8 to 1, calling their relation in ordinary steam 1 to 1. A considerable engine could thus also be brought within a very moderate compass. Possibly, after a few operations of the air pump, the incondensible gas may be so effectually withdrawn, that we might be permitted to detach this mechanism, which, though essential to common engines, takes away one fourth of their power. In a distillery in this country, or on a sugar estate in the colonies, a trial of this plan might perhaps be made with advantage. While exercising its mechanical functions of grinding, mashing or squeezing the canes, it would be converting ordinary into strong spirit for rectification, or for

the convenience of carriage. Might not such an engine be executed on a small scale, for many purposes of domestic drudgery? It would unquestionably furnish a beautiful illustration in philosophy, to make one small portion of liquid, by the agency of fire, imitate the ceaseless circulation and restless activity of life.

XIX. *Observations on the heights of mountains in the north of England. By* Thomas Greatorex, *Esq. F. L. S. In a letter to* Thomas Young, *M. D. For. Sec. R. S.*

Read May 7, 1818.

HAVING been desirous, for many years past, to revisit the lakes in the north of England, and wishing, when I should be able to take that tour, to make some experiments on one of the mountains in that district, I applied to the late Mr. RAMSDEN, who made for me the following instruments:

A mountain barometer.

A stationary do. (the mercury of both boiled in the tube.)

A telescope with cross-wires, and a level fitted to it, mounted on a tripod-staff.

And a small theodolite, with compass, &c.

These he assured me were made with the greatest care.

I have this summer passed some weeks in the vicinity of the Lakes; and at Keswick I fortunately met with one of those superior self-taught geniuses, not uncommon in the North, who entered into all my views, and proved eminently serviceable to me. His name is OTLEY, and he is a watch-maker, but acts occasionally as guide up the mountains, &c.

As I wished to measure Skiddaw geometrically down to the average level of Derwent Water, I had a tapering staff made about 28 feet long, shod with an iron point, and very carefully graduated from an accurate standard yard measure, sent from the proper office, in London.

The graduation commenced from a zero about 3 feet from the bottom of the staff, so that from the zero to its top was exactly 25 feet; the top ending in a bluntish point.

Twelve feet six inches of the upper part of the staff could occasionally be separated from the lower part, both for the convenience of carrying, and in case the wind should prevent the use of the whole length. Small cords were also attached to it, for the purpose of holding it steady and perpendicular.

We first determined the height of OTLEY's house above the lake, which was 10 yards, and at this height the stationary barometer was placed; and Mr. CROSTHWAITE, of the Keswick Museum, undertook to note its variation and that of the thermometer every half hour.

We then ascended Skiddaw, accompanied by Mr. AIREY, a black-lead pencil maker (who proved an excellent assistant), and two boys. On the summit the barometer was set up. I first adjusted it, and privately noted down the height of the mercury; then purposely deranged it, to let OTLEY re-adjust it, and write down his observation, which never differed from mine more than one thousandth of an inch during the whole series of observations. The heights of the attached and detached thermometers being also noted, we proceeded to the measurement by the level and staff.

The telescope being levelled, and its cross wires intersecting the highest point of the mountain, it was then pointed in the direction of the most convenient descent, and the staff carried down the hill till its top exactly coincided with the cross wires, the level of the telescope being carefully preserved; the perpendicularity of the staff was ascertained by

plumb lines. And here I must mention how greatly we were favoured by the weather: the sun only shone during the first observation, and it was nearly a calm during the whole of the measurement, so that AIREY could frequently, by inserting the pole a little in the ground, set it on a balance, which would remain during the time necessary for adjusting the level, and observing it.

I have also to observe, that we chose so precipitous a descent, that the pole was seldom 40 feet from the telescope, therefore no allowance was necessary for the earth's curvature.

I found the most expeditious and exact mode of managing the pole, was to stop Mr. AIREY, when I perceived its top to be about an inch above the cross wires; as I could then make signs to him to press it into the earth by little and little till the coincidence was exact: the telescope was then carried down to the pole, and placed (when levelled) in exact correspondence with the zero. The pole was again removed down to a new station, and this mode continued till we had compleated six observations, or 50 yards of descent. Here the barometer was again set up, separately examined as before; and this process continued to the foot of the mountain.

After descending 175 yards, it was necessary to take a dead level of half a mile, which was corrected by several observations back and forward, as also by a middle station.

We measured 400 yards down the first day, and on the following day compleated 900 yards, which brought us to the village of Applethwaite, and near the level of Bristow Hill, about a mile distant.

The next day many observations were made back and

forward between Armathwaite and Bristow Hill, allowing 8 inches for the earth's curvature: a small allowance appeared necessary for refraction, the object appearing rather lower than it ought to have been. (This was in the forenoon.)

From Bristow Hill, by means of a middle station, a level was taken to Crow Park, close to the lake, and the measurement compleated down to the mean height of Derwent Water.

Results of the levelling.

	yds.	ft.	in.
From the summit of Skiddaw to Applethwaite	900	0	0
Down to the level of Bristow Hill - - -	5	0	7½
Thence to the level of Crow Park - - -	14	2	5
Down to the average height of Derwent Water -	16	0	3
Skiddaw above the lake - -	936	0	5½
Derwent Water above the sea at low-water mark, according to an accurate measurement by the late Mr. CROSTHWAITE -	76	0	0
Height of Skiddaw by levelling -	1012	0	5½

Results of the several barometrical observations on the summit of Skiddaw, Sept. 15th. 1817, at 10 hours 50 min. A. M.

	inches.		
Barometer below, 10 yds. above the lake	30.050.	Ther. 61°.	
Barometer above - -	27.156.	Th. attached 57°.	Do. detached 50°.

	yds.
Whence, by Dr. MASKELYNE's formula, the height - -	= 926.1685
Measured height - - - -	= 926
error +	.1685
By Dr. HUTTON's formula - -	925.285
error —	.715

50 yards down. 12 h. 20 m.

Bar. below - -	30.030.	Ther. 62°.	
Do. above - -	27.310.	Do. 56°.*	

	yds.
Measured height - -	876
By Dr. MASKELYNE -	873.194477
error —	2.805523
By Dr. HUTTON - error —	3.945

* The attached and detached thermometers did not differ for the remainder of the observations.

100 yards down. 1 h. 10m.

		inches.	
Bar. below	-	30.010. Ther. 62°.	
Do. above	-	27.444. Do. 56°.	yds.
Measured height		-	826
By Dr. MASKELYNE		-	821.71247
		error —	4.28753
By Dr. HUTTON		error —	4.6032

150 yards down. 1 h. 55m.

Bar. below	-	30.010. Ther. 63°.	
Do. above	-	27.590. Do. 56°.	yds.
Measured height		-	776
By Dr. MASKELYNE		-	772.3057
		error —	3.6943
By Dr. HUTTON		error —	4.0444

200 yards down. 4h. 20m.

Bar. below	-	30. Ther. 61°.	
Do. above	-	27.708. Do. 53°.	yds.
Measured height		-	726.
By Dr. MASKELYNE		-	724.7245
		error —	1.2755
By Dr. HUTTON		error —	2.092

250 yards down. 4h. 52m.

Bar. below	-	30. Ther. 60°.	
Do. above	-	27.851. Do. 50°.	yds.
Measured height		-	676.
By Dr. MASKELYNE		-	672.252
		error —	3.748
By Dr. HUTTON		error —	4.3152

300 yards down. 5h. 50m.

Bar. below	-	29.990. Ther. 58°.	
Do. above	-	28.002. Do. 48°.	yds.
Measured height		-	626.
By Dr. MASKELYNE		-	616.731
		error —	9.269
By Dr. HUTTON		error —	9.6532

350 yards down. 6h. 45m.

Bar. below	-	29.987. Ther. 57°.	
Do. above	-	28.250. Do. 47°.	yds.
Measured height		-	576.
By Dr. MASKELYNE		-	566.3645
		error —	9.6355
By Dr. HUTTON		error —	9.91

400 yards down. 7h. 15m.

	inches.	
Bar. below	29.980. Ther. 55°.	
Do. above	28.310. Do. 47°.	yds.
	Measured height	526.
	By Dr. MASKELYNE	513.3128
	error —	12.6872
	By Dr. HUTTON — error —	12.84

400 yards down. Sept. 6th, 1817. 10h. 15m. A. M.

Bar. below	29.890. Ther. 59°.	
Do. above	28.240. Do. 54°.	yds.
	Measured height	526.
	By Dr. MASKELYNE	517.89753
	error —	8.10247
	By Dr. HUTTON — error —	8.674

450 yards down. 11h. 5m.

Bar. below	29.900. Ther. 59°.	
Do. above	28.396. Do. 54°.	yds.
	Measured height	476.
	By Dr. MASKELYNE	470.2667
	error —	5.7333
	By Dr. HUTTON — error —	6.2514

500 yards down. 11h. 54m.

Bar. below	29.906. Ther. 61°.	
Do. above	28.564. Do. 56°.	yds.
	Measured height	426.
	By MASKELYNE	417.8833
	error —	8.1167
	By Dr. HUTTON — error —	8.65

550 yards down. 12h. 24m.

Bar. below	29.905. Ther. 61°.	
Do. above	28.730. Do. 56°.	yds.
	Measured height	376.
	By Dr. MASKELYNE	365.8427
	error —	10.1573
	By Dr. HUTTON — error —	10.6186

600 yards down. 12h. 53m.

Bar. below	29.908. Ther. 61°.	
Do. above	28.866. Do. 56°.	yds.
	Measured height	326.
	By Dr. MASKELYNE	323.098758
	error —	2.901242
	By Dr. HUTTON — error —	3.326

650 yards down. 2h. 5m.

	inches.	
Bar. below	29.910. Ther. 60°.	
Do. above	29.046. Do. 56°.	yds.
Measured height		276.
By Dr. Maskelyne		266.8915
	error —	9.1085
By Dr. Hutton	error —	9.446

700 yards down. 2h. 38m.

Bar. below	29.910. Ther. 60°.	
Do. above	29.188. Do. 56°.	yds.
Measured height		226
By Dr. Maskelyne		221.8442
	error —	4.1558
By Dr. Hutton	error —	4.446

750 yards down. 3h. 5m.

Bar. below	29.920. Ther. 59°.	
Do. above	29.364. Do. 57°.	yds.
Measured height		176.
By Dr. Maskelyne		171.333
	error —	4.667
By Dr. Hutton	error —	4.888

800 yards down. 3h. 35m.

Bar. below	29.930. Ther. 59°.	
Do. above	29.548. Do. 58°.	yds.
Measured height		126.
By Dr. Maskelyne		117.82
	error —	8.18
By Dr. Hutton	error —	8.2996

850 yards down. 4h. 5m.

Bar. below	29.940. Ther. 58°.	
Do. above	29.716. Do. 58°.	yds.
Measured height		76.
By Dr. Maskelyne		69.36818
	error —	6.63182
By Dr. Hutton	error —	6.4918

900 yards down. 4h. 48m.

Bar. below	29.945. Ther. 58°.	
Do. above	29.850. Do. 58°.	yds.
Measured height		26.
By Dr. Maskelyne		29.351
	error +	3.351
By Dr. Hutton	error +	3.313

From the near agreement of the measured and barometrical heights on the summit of Skiddaw, I had formed sanguine hopes that the barometer would prove a most exact determinator of altitudes, and almost supersede the necessity of having recourse to any other mode; but the subsequent observations lead me to fear that the state of the atmosphere has an effect which we cannot yet account for, and to which we cannot apply a correction. When the operation was discontinued on the evening of the 5th, the air was dry and clear; but on the morning of the 6th, the top of the mountain was clouded about 300 yards down, which might be the reason of the two observations differing by more than four yards.

But the object of this paper is not to surmise, or submit any hypothesis; but faithfully to state the result of operations carried on with all the exactness I was capable of. If what I have done be deemed worthy of any attention by the learned and distinguished Society of which you, Sir, are a member, I shall be most highly gratified, and amply compensated for the toil attending operations on high mountains.

THOMAS GREATOREX.

P. S. Dr. MASKELYNE's rules for determining the height of mountains by the barometer, are these:

1st. Take the difference of the tabular logarithms of the observed barometrical heights at the two stations, considering the four first figures (exclusive of the index) as whole numbers, and the remaining figures to the right as decimals.

2dly. Observe the difference of FAHRENHEIT's thermometer

at the two stations; multiply this difference by $\frac{454}{1000}$, and add or subtract this product, according as the thermometer was highest at the upper or lower station, which will give an approximate height.

3dly. Take the mean of the two altitudes of the thermometer, and find the difference between this mean and 32°. Multiply the approximate height by this difference, and the product by the decimal fraction .00244. This last correction being added to, or subtracted from the approximate height, according as the mean of the two altitudes of FAHRENHEIT's thermometer was greater or less than 32°, will give the true height of the upper station in English fathoms.

Dr. HUTTON's *rules.*

1st. Let the heights of the barometer at the top and bottom of any elevation intended to be measured, be observed as near the same time as may be, as also the temperatures of the attached thermometers, and also the temperature of the air in the shade at both stations, by means of detached thermometers.

2dly. Reduce these altitudes of the barometer to the same temperature by augmenting the height of the mercury in the colder temperature, or diminishing that in the warmer by its $\frac{1}{1000}$ part for every degree of difference of the two.

3dly. Take the difference of the common logarithms of the two heights of the barometer (so corrected), considering the four first figures (exclusive of the index) as whole numbers, and the rest to the right as decimals, which will give an approximate height.

4thly. Take the mean of the two detached thermometers; and for every degree which this differs from 31°, take so many times the $\frac{1}{435}$ part of the approximate height; and *add* them if the mean temperature be *above* 31°; but *subtract* them if it be *below* 31°; and the sum or difference will be the true altitude in fathoms.

XX. *On the different methods of constructing a catalogue of fixed stars.* By J. Pond, *Esq. F. R. S. Astronomer Royal.*

Read May 21, 1818.

In the present state of practical astronomy, the principal object in a national observatory, such as that at Greenwich, is to define the position in the heavens, of the fixed stars and other celestial bodies, at the moment of their passage over the meridian; and we judge of the perfection of the instruments, and of the skill with which they are employed, by the degree of precision with which this operation is performed.

The very great changes and improvements that have taken place within these few years in the instruments of this establishment, are well known to all persons who have interested themselves in its concerns. The liberality of his Majesty's Government has been literally unbounded; and instruments of unusual magnitude and of most difficult construction, have been executed with a success that has exceeded the most sanguine expectation. Such powerful means entrusted to my care, could not but produce, on my part, a continued anxiety that they should be employed in the most advantageous manner. Being constantly under the necessity of reflecting a great deal on the various possible modes, both of making observations and of deducing results from them, I have insensibly been led to adopt methods which differ very materially from those generally pursued,

and some of these appear to me to possess advantages so decided, that I venture to submit them to the consideration of this Society; at the same time, conscious that the subject cannot excite a very general interest, I shall endeavour to abstain as much as possible from every unnecessary detail, and confine myself to the explanation of the general principles of the method which I propose to recommend.

According to the method hitherto invariably followed in this Observatory in constructing a catalogue of stars, either in declination or right ascension, some one star has been taken as a point of departure, and the positions of all the rest determined by direct comparison with this alone. The declinations were determined by direct measurement with γ Draconis;* and α Aquilæ was chosen as the common term of comparison in right ascension. This mode of proceeding with the mural quadrant, though evidently capable of improvement, was not so very objectionable, as when applied to the observations made with the transit instrument; as the observer must be supposed desirous of obtaining the greatest possible accuracy from a given number of observations.

Indeed, in the latter case, the principle is so very objectionable, that I cannot now help expressing some surprize that it should have been employed for so great a length of time. In the first place, every result deduced from the observation of each star is affected with a double error; that committed in the observation of α Aquilæ, and that in the observation

* This will evidently appear to have been the case, if it be considered that the plumb line in the mural quadrant performed no other office than that of maintaining the instrument in a given position.

of the star itself; this objection however is but trifling, compared with a more important one, which is this: if the observation of α Aquilæ be omitted, either from bad weather, or from its passing the meridian at an inconvenient hour, or from neglect, then the observations of all the other stars are, for the purposes of this investigation, rendered entirely useless. Hence arose the necessity of combining the observations of so many years, to construct a catalogue with the accuracy required. That extreme accuracy was ultimately obtained, I am most happy to have it in my power to affirm, since the difference is almost insensible between the catalogue lately deduced from the new transit instrument and that of my predecessor; though an interval of ten years has intervened between the periods of their construction. The advantage therefore of my method I conceive to consist in this; that a catalogue may be constructed from the observations of a single year, equal in accuracy to one which formerly was obtained in three.

The method I propose is equally applicable to the mural circle and the transit instrument. With neither do I assume any particular star as a point of departure, in preference to the rest; but, on the contrary, every star in its turn is assumed as a point of reference to the others; thus endeavouring, in the first instance, to establish their relative distances from each other on the equator, or meridian, leaving the choice and determination of some common point of departure as a subject for future consideration.

To render this more easily intelligible, perhaps it would be better to consider each instrument separately.

Whoever attentively examines the construction of the mural circle at Greenwich, will perceive that its operation is entirely limited to the measurement of the meridional distance between different stars. It is, in fact, a theodolite placed vertically. By an extension of its principle, it can measure the distance between a star and its reflected image from a mercurial horizon, and thus determine the altitude of a star; and, in common with every other circle, it can measure the distance of a circumpolar star from the pole by an observation of its inferior and superior passage over the meridian. As the instrument cannot be reversed, a plumb line could not be applied, with any advantage. In circles that turn freely in azimuth, a double observation of a star gives the angle which the star makes with the axis of the instrument round which it turns, and a plumb line is properly applied in this case to ensure the verticality of this axis; but on the Greenwich circle, a plumb line could only serve as a point of departure; and this having no reference to the real zenith, would, in my opinion, be very injudiciously chosen.

Let us suppose, by way of example, that twelve stars had been observed with the mural circle, and one of these the pole star; moreover, that this star had been observed below the pole. Then, as it would be easy to determine the polar point, the position of all the remaining stars, with reference to this point, might likewise be ascertained. It is however evident in this case, that each result would be affected by a double error, viz. the error committed in the determination of the polar point, and also that due to the observation of each star.

Let us next suppose that, from some accidental cause, the inferior passage of the pole star had been omitted; then, if the declination of this star be supposed to be known from previous observation, the polar point may be deduced from this supposition; but the pole star, in this case, would have no superiority over any other star, whose polar distance should previously have been determined. If we employ two stars equally well known, it is evident that greater accuracy will be obtained; and it is easy to conceive that by taking a greater number of stars, for instance the whole twelve, we may obtain greater accuracy than even by a double observation of the pole star, because the error of observation, and that arising from false assumption of polar distance, will necessarily be much diminished by the natural tendency which the positive and negative errors will have to counteract each other.

These considerations, aided by daily trial and experience, induced me very soon after the mural circle was erected, to abandon every method which assumed one particular point of departure, and I directed all my efforts to the determination of the difference of declination which existed between every star and all the rest; constructing by this means a catalogue, which, with respect to the pole or the zenith, might be subject to a common error; reserving to myself the power of investigating this common error by a process which will afterwards be explained. In the practical execution of this method, the observation of each star is employed for a double purpose; it first serves, in combination with all the others, to find the common index error; this common error again applied to the individual observation, gives a

new result of the position of the star: and of the totality of these results is the catalogue ultimately formed.

In a paper which I had the honour of communicating to the Society, in the year 1816, I have given several examples of the manner in which the index error of the mural circle is calculated, though I did not at that time explain the principle or advantages of this method. In the printed Greenwich observations will be also found the computations of this index error for every month.

I should observe, that when the greatest possible accuracy is required, the whole of the observations should be re-computed from the catalogue now supposed to be in its most perfect state, except the extremely small improvement it may receive from this process. And thus, by continued observation and approximation, may the *relative* places of the stars be assigned to a very unexpected degree of accuracy. Now, though a continuation of this process has an evident tendency to improve the catalogue, as far as concerns the relative distances of the several stars to each other, yet it can have no effect in correcting any common error that may exist from a false assumption either of the zenith or the pole. I will endeavour, therefore, to explain the next part of the process, which is to determine the common error in polar distance. It is to be presumed that the pole star forms one of the stars of which the intended catalogue is composed, and it should be observed most assiduously, both above and below the pole. These observations are to be treated as if appertaining to different stars, and the place of each determined in the catalogue, by applying the same index error as that em-

ployed for the other stars. When the whole catalogue is completed, these two results are to be examined; and, if they appear equally distant from the pole, the catalogue is affected by no common error; but if, on the contrary, the polar point is not found to be precisely between these results, then half the difference will be the common error.

The details of these computations will be found in the Greenwich observations; and it will there be seen that the polar distances of this star, determined by 158 observations, in the same manner as any other star of the catalogue, namely, by the application of the common index error, was found to be - - - 1.42.21.50

Below the pole, by 132 observations, computed in the same manner = 358.18.38.32

Their sum - - 359.59.59.82

360

Difference from 360 - 0 0 0″.18

Difference or common error = 0.09

This difference, 0″.09, is the common error to be added to each star of the catalogue, and the polar distance of the star thus corrected will be 1.41.21.59.

I find by about 330 observations, deduced in the usual manner, without any reference to the other stars, 1.41.21.65, reduced to the beginning of 1813.

The same principle, which I have thus attempted to explain, may be applied with equal facility, and even greater advantage, to the formation of a catalogue in right ascension. The

error of the clock in this case, answers to the index error of the circle, and is investigated in precisely the same manner. An approximate catalogue is first assumed; and here, as with the circle, the observation of each star serves a double purpose: in the first place, in common with all the rest, it is employed to determine the error of the clock; and the error thus found is applied to the observation of the star. In this case, the right ascension of the star is supposed to be known; in the second part of the process, the star is considered as a planet, or unknown object, and its right ascension found by the usual rules, and recorded as a single result; and from the totality of these results is the right ascension ultimately obtained.

Should the deduced right ascensions differ materially from those originally assumed, the improved catalogue must be substituted for the approximate one, and the whole process recomputed. In this substitution, however, some judgment and discretionary power must be exercised; for should the assumed or approximate catalogue be very exact, and the subsequent observations few or inaccurate, it is evident that the new catalogue might be less correct than the assumed one; this, however, does not arise from any defect in the method, but is the inevitable consequence of any attempt to improve good observations by bad ones. As a practical illustration of this remark, I might add, that having myself assumed an approximate catalogue, so exact as that of Dr. MASKELYNE, it happens that at this moment, in the case of some stars which have not been very frequently observed, I have great doubts whether the new determination is more

exact than the old one; but by continuing this process, a proper ascendancy will necessarily be acquired by the latest observations.

In comparing my catalogue of right ascensions with that of Dr. MASKELYNE, I meet with a singular coincidence, which seems to me to illustrate and confirm, in a very striking manner, the advantage of the principle in question. In each catalogue, the right ascension of α Aquilæ, though deduced apparently by a different process, comes out the same, even to the hundredth part of a second. Accident may possibly have some little share in this very exact coincidence, but it appears to me chiefly to arise from the very nature of our respective processes. In Dr. MASKELYNE's method, the right ascension of every star is derived from direct comparison with α Aquilæ, or in other words, the right ascension of α Aquilæ is derived by comparing it with every star. So it is in my method; and hence the same result ought to be obtained. But the advantage which in one case is peculiar to α Aquilæ, is in the other method extended to every star: no possible reason can be given, why the place of one star should be more accurately assigned than that of another, provided an equal number of observations be obtained of each, since the place of every star is deduced exactly in the same manner from a comparison of all the rest.*

Though not immediately connected with the present subject, I wish to take this opportunity of stating, that, in comparing the observations of the old transit instrument with those of the new one, I find a much less difference than I

* As I have nothing new to offer, as to the method of deducing the equinoctial point from observations of the sun, I have not taken this part of the subject into consideration.

expected. It appears to me that the former instrument must have described nearly a correct hour circle, though this circle was evidently not exactly the meridian. With all the decided superiority of the new instrument, I cannot venture to assign to the catalogue of my predecessor an error much greater than one tenth of a second of time, even in the stars near the horizon, where the error appears probably to have been the greatest. I trust it will be considered by astronomers, as creditable to the history of this Observatory, that two observers, with different instruments (and by as different a method of computation as the case admits of) should deduce two catalogues so exactly alike, that they may be considered almost as identical.

It is an interesting question to every astronomer possessed of a valuable instrument, to know to what degree of accuracy its results can be depended on. I have examined a great number of observations made with the new transit instrument with this view, and it appears to me, that near the equator, 60 observations will generally give the second decimal place of a second of time very correctly; 120 observations will give this with greater certainty, but not in the proportion of two to one.* This I think is rather a greater exactness than can be obtained by the mural circle,† and the reason I apprehend to be this.

* I find, by the rule given by M. LAPLACE, that the probable error of 120 observations is $0^s.005$ in time.

† The optical power of the new transit instrument is so decidedly greater than in the former one, that each observation must necessarily be more exact; but I do not find the discordances in the ultimate results smaller in the same proportion: from this circumstance I conclude, that the limit to accuracy consists rather in the clock, than in the instrument.

The Greenwich transit clock, compared with others, is, I believe, considered as a

With the mural circle, or any similar instrument, the place of a star appears to be defined at the moment of observation with much greater precision than by a transit instrument; but the error of the latter, I conceive, to be much more purely accidental; so that the result of a great number of observations will be more exact. Local refraction places an insurmountable limit to observations in declination, particularly at any sensible distance from the zenith: observations in right ascension are free from this material source of error, and are therefore susceptible of attaining greater accuracy by continued perseverance.

I have no doubt but to many persons, and even to those extremely conversant with the theory of astronomy, it may appear, that a very undue degree of importance is attached by practical astronomers to the investigation of such minute quantities as form the subject of this paper; and it is a question often asked, of what importance can it be to science, that the place of a fixed star should be so accurately ascertained? A person in my situation might reply, that being employed to do this, it is incumbent on him to do it in the best manner possible, leaving the question of utility to others; but there is really a very reasonable and satisfactory answer to be given to this question. In the first place, since the position of the sun, moon, and planets, are deduced from direct comparison with the principal stars, it is requisite, for this purpose, that their places should be accurately known, and their various changes ascertained. Moreover,

very good one; but of all the instruments in the Observatory, it is certainly that in which improvement would be most beneficial.

though in common language we speak of determining the places of the fixed stars relatively to the equator and ecliptic, the real fact is, that the stars being stationary, it is the situation of these circles themselves, and the point of their intersection, that is the ultimate object of research; and in this point of view we at once perceive how these precise determinations may facilitate the most abstruse investigations of physical astronomy.

XXI. *A description of the teeth of the Delphinus Gangeticus.* By *Sir* Everard Home, *Bart. V. P. R. S.*

Read June 4, 1818.

THE Delphinus Gangeticus is described by the late Dr. ROXBURGH, in the seventh volume of the Asiatick Researches, published in 1781; but no farther account is given of its teeth, than that the number is 120, 30 in each jaw, nor have I met with any description of them in other publications. Dr. SHAW published his second volume of General Zoology, in which the whale tribe is mentioned, in 1801. He says, a narrow snouted dolphin is supposed to inhabit the Indian seas; but it is only known to us from specimens of the head and jaws. The jaws are extremely narrow, the teeth small, not numerous, distant, and shaped somewhat like the molares of quadrupeds. This description corresponds so ill with the teeth of the Delphinus Gangeticus, that it would almost induce us to believe that it is meant for those of another animal.

A specimen of the upper and lower jaw of the Delphinus Gangeticus was given to me, 17 years ago, by Sir JOSEPH BANKS, and has been deposited ever since in the HUNTERIAN Collection; but it was only the other day that an accidental reference to the Asiatick Researches led us to discover the animal to which they belong. The singularity of the teeth made it always a remarkable object, and now the animal is known, a description of them may not be undeserving the attention of this Society.

The jaws and teeth form the most remarkable characters of this species of delphinus, and a description of them will not only interest the naturalist and comparative anatomist, but will enable the geologist, when fossil teeth are found of this shape, readily to determine the species of animal to which they had belonged.

These teeth, as is common in those of the whale tribe, have the first rudiments formed in the gum, from which the tooth grows in both directions, upwards through the gum in the form of the point of a flattened cone which is coated with enamel, and downwards towards the jaw, increasing considerably in breadth but not in thickness, till it is at last embedded in the substance of the jaw itself; the lower portion has no enamel.

The appearance the teeth put on, as well as the mode of growth, will be seen in the annexed drawing; [Pl. xx.] the change that takes place in the form of the tooth as it wears away from long use, is more remarkable than in most other teeth; for the perfect tooth has a tolerably sharp enamelled point, while the half worn one has a curved blunted cutting edge. The teeth in front of the jaw are more like the incisores in other animals. The whole number in both jaws is 120, which exactly corresponds with that given by Dr. ROXBURGH, and identifies these jaws as belonging to the animal which he has described.

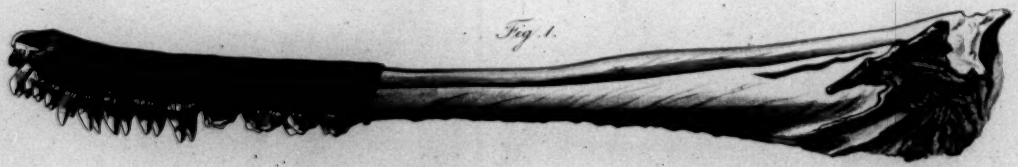


Fig. 1.

Fig. 3.

Fig. 4.

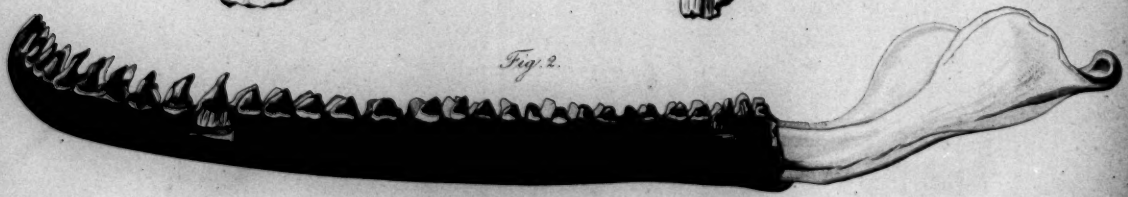


Fig. 2.

Philos. Trans. MDCCCXVII. Part. XI. p. 254.

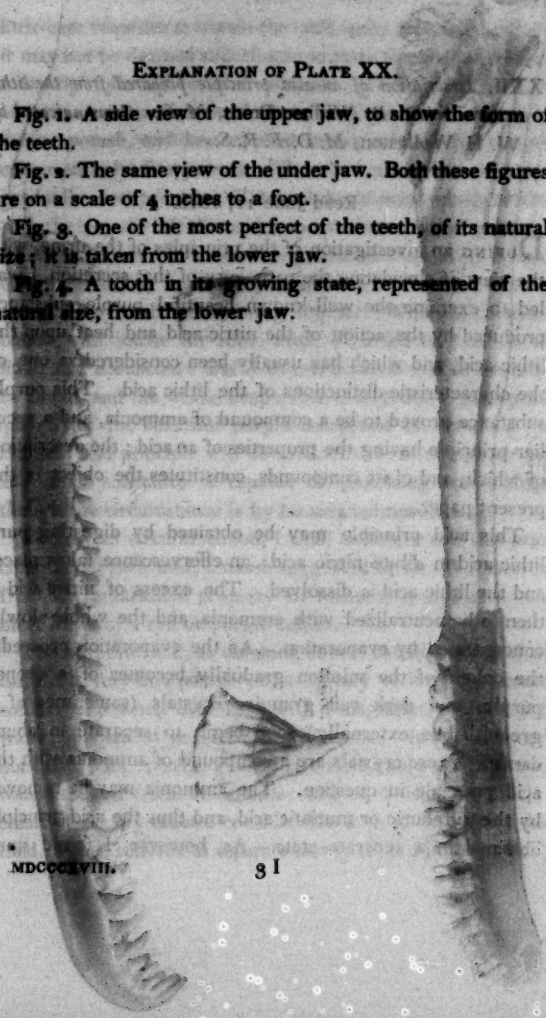
Explanation of Plate XX.

Fig. 1. A side view of the upper jaw, to show the form of the teeth.

Fig. 2. The same view of the under jaw. Both these figures are on a scale of 4 inches to a foot.

Fig. 3. One of the most perfect of the teeth, of its natural size; it is taken from the lower jaw.

Fig. 4. A tooth in its growing state, represented of the natural size, from the lower jaw.



XXII. *Description of an acid principle prepared from the lithic or uric acid.* *By* William Prout, *M. D.* *Communicated by* W. H. Wollaston, *M. D. F. R. S.*

Read June 11, 1818.

During an investigation of the principles of the urine, with the view of elucidating the pathology of that secretion, I was led to examine the well-known beautiful purple substance produced by the action of the nitric acid and heat upon the lithic acid, and which has usually been considered as one of the characteristic distinctions of the lithic acid. This purple substance proved to be a compound of ammonia, and a peculiar principle having the properties of an acid; the description of which, and of its compounds, constitutes the object of the present paper.

This acid principle may be obtained by digesting pure lithic acid in dilute nitric acid: an effervescence takes place, and the lithic acid is dissolved. The excess of nitric acid is then to be neutralized with ammonia, and the whole slowly concentrated by evaporation. As the evaporation proceeds, the colour of the solution gradually becomes of a deeper purple, and dark red granular crystals (sometimes of a greenish hue externally) soon begin to separate in abundance. These crystals are a compound of ammonia with the acid principle in question. The ammonia may be removed by the sulphuric or muriatic acid, and thus the acid principle obtained in a separate state. As, however, I found some

little care requisite to obtain the acid quite free from colour, it may not be deemed superfluous to state the precise method I usually followed for that purpose. The compound with ammonia, above-mentioned, was dissolved in a solution of caustic potash, and heat applied to the solution till the red colour entirely disappeared. This alkaline solution was then gradually dropped into dilute sulphuric acid, which uniting with the potash, left the acid principle in a state of purity.

The acid principle is likewise produced from lithic acid by chlorine. Iodine has also the same remarkable property, though in a much less striking degree. When lithic acid is boiled with iodine for some time, a partial solution of the lithic acid is effected; and if to this solution a little ammonia be added, and the whole evaporated to dryness, a perceptible quantity of the beautiful purple compound of ammonia and the new acid principle will be obtained. I am not aware that any other substance is capable of producing this change, though the circumstance is by no means improbable.

To prevent circumlocution, I shall in future call this principle the *purpuric acid*, a name suggested by Dr. WOLLASTON, from its remarkable property of forming compounds with most bases of a red or purple colour.

The purpuric acid, as obtained above, usually exists in the form of a very fine powder, of a slightly yellowish or cream colour; and when examined with a magnifier, especially in water, appears to possess a pearly lustre. It has no smell nor taste. Its specific gravity is considerably above that of water, though from the minute state of division in which it exists, it usually takes a considerable time to subside in that fluid. When suffered to separate slowly from a large quantity

of water, or any other fluid capable of holding it in solution, it sometimes assumes the form of thin pearly scales.

The purpuric acid is very little soluble in water. One tenth of a grain boiled for a considerable time in 1000 grains of water was not entirely dissolved. The water assumed a purple tint, which it retained after it was cold, though it became very slightly turbid on cooling.* The purpuric acid is insoluble in alcohol and ether. In all the mineral acids, when concentrated, and in excess, and in solutions of the different alkalies, it dissolves readily; but it is insoluble, or nearly so, in dilute sulphuric, muriatic, and phosphoric acids, and also in solutions of the oxalic, citric, and tartaric acids. Concentrated nitric acid readily dissolves it with effervescence; and if the acid be in excess, and heat be applied, a portion of the purpuric acid is decomposed, ammonia is formed, and on driving off the excess of nitric acid by heat, the purpurate of ammonia is obtained, precisely as if a little of the lithic acid had been treated in a similar manner. Chlorine, likewise, dissolves the purpuric acid, and apparently produces the same changes upon it as the nitric acid. It readily dissolves also by the assistance of heat, in concentrated acetic acid.

The purpuric acid does not sensibly affect litmus paper, probably on account of its insoluble nature. When exposed to the air it does not deliquesce, but gradually assumes a purplish tint, apparently by attracting a little ammonia from

* I am not quite sure whether the purple tint here mentioned depends upon the actual solution of a minute portion of the purpuric acid; and, consequently, whether it naturally forms a purple solution, or whether the colour be owing to the formation of a little ammonia from the decomposition of a minute proportion of the acid, which, combining with the remainder of the acid, forms the purpurate of ammonia. I incline to the latter opinion.

the atmosphere, or perhaps from the evolution from itself of a little of the same alkali by spontaneous decomposition.

Submitted to heat, it neither melts nor sublimes, but acquires a purple hue from the formation of ammonia, and afterwards burns gradually, without yielding any remarkable odour. Subjected alone to heat in close vessels, it yields a considerable proportion of the carbonate of ammonia, some prussic acid, and a little fluid having an oily appearance; while a portion of pulverulent charcoal remains. When given quantities were burnt with the oxide of copper, in the manner formerly described by me,* data were obtained, which appeared to show that one hundred parts consist of

Hydrogen	4.54	corresponding with 2 atoms or proportions.
Carbon	27.27	2 ditto.
Oxygen	36.36	2 ditto.
Azote	31.81	1 ditto.

The purpuric acid combines with the alkalies, alkaline earths, and metallic oxides. It is capable of expelling the carbonic acid from the alkaline carbonates, by the assistance of heat, and does not, as far as I have observed, combine with any other acid. These are circumstances sufficient, as Dr. Wollaston has observed, to distinguish it from an *oxide*, and to establish its character as an *acid*. On the supposition then, that it be named the *purpuric acid*, its compounds with different bases must be denominated *purpurates*: on some of the most remarkable of which I shall now proceed to make a few remarks.

Purpurate of ammonia. This salt crystallizes in quadrangular prisms, which, when viewed by transmitted light, are

* See Medico-Chirurgical Transactions, vol. viii, p. 526.

transparent, and of a deep garnet red colour; but by reflected light, their two broadest opposite faces appear of a brilliant green, closely resembling that of the wings of some of the beetle tribe, as for example, of the *Cetonia aurata*, while their other two opposite faces appear of a dull reddish brown colour; or if the light be very strong, slightly green. This peculiarity seems to be possessed in a greater or less degree by all the other alkaline, and perhaps earthy salts; and doubtless depends upon the structure of the crystals. The purpurate of ammonia is soluble in about 1500 parts of water at 60°, but in boiling water is much more soluble. The solution is of a beautiful deep carmine, or rose red colour. In pure alcohol and in ether, it is little if at all soluble. The aqueous solution has a slightly sweetish taste, but no smell. By adding this aqueous solution of the purpurate of ammonia to neutral saline solutions of other bases, most of the following purpurates were formed.

Purpurate of potash. When a saturated boiling solution of the purpurate of ammonia is added to a solution of the bicarbonate of potash, a dark brownish red precipitate takes place, which is the purpurate of potash. If, however, this salt be slowly formed, it may be obtained in a crystalline form; and the crystals appear to possess the same peculiarity with respect to colour, as those of the purpurate of ammonia above-mentioned. This salt is much more soluble than the purpurate of ammonia.

Purpurate of soda. This salt, when obtained by the same means as the purpurate of potash, is of a dark brick red colour. It may, however, be obtained in crystals. It is much less soluble than the purpurate of potash. Three thousand times its weight of water at 60°, did not completely dissolve

it. The colours of the solutions of this salt, and of potash, differ slightly from one another, and also from that of the purpurate of ammonia; but it is not easy to describe these differences so as to render them intelligible.

Purpurate of lime. This salt, when obtained by adding a boiling saturated solution of the purpurate of ammonia to a solution of the muriate of lime, exists in the form of a powder much resembling in colour the crust of the lobster before it is boiled. This salt is but little soluble in cold water; but in boiling water it is more soluble, and the solution is of a beautiful reddish purple colour.

Purpurate of strontian. This salt obtained as above, with the nitrate of strontian, exists in the state of a dark brownish red powder, with a slight tinge of green. It seems to be more soluble than the purpurate of lime, and forms a purple solution.

Purpurate of barytes. Obtained as before described, with the acetate of barytes, this salt assumes the form of a dark green powder, not apparently differing much in point of solubility from the purpurate of strontian; and forming, like that salt, a purple solution.

Purpurate of magnesia. This is a very soluble salt. Its solution is of a beautiful purple.

Purpurate of alumina. When a solution of the purpurate of ammonia was added to a solution of alum, no perceptible change took place immediately; but after some time the colour of the solution disappeared, and a small quantity of a white substance separated, which was presumed to be the purpurate of alumina, but it was not examined.

Purpurate of gold. When a solution of the muriate of gold is dropped into a solution of the purpurate of ammonia, the

colour becomes yellowish, but no precipitation takes place. Hence, this salt may be presumed to be very soluble.

Purpurate of platina. The muriate of platina changes the colour of the purpurate of ammonia to a yellowish scarlet, but produces no precipitation.

Purpurate of silver. Solutions of the acetate or nitrate of silver, dropped into a solution of the purpurate of ammonia, produce a deep purple precipitate; and the water is left nearly colourless. Hence the purpurate of silver appears very insoluble.

Purpurate of mercury. A solution of the proto-nitrate of mercury produces, with the purpurate of ammonia, a beautiful reddish purple precipitate, and the water is left nearly colourless. A solution of the oxymuriate of mercury produces at first no change; but after some time a copious light rose coloured precipitate occurs, and the solution is left colourless.

Purpurate of lead. A solution of the nitrate of lead, dropped in a solution of the purpurate of ammonia, renders it of a rose red colour; but no precipitation takes place.

Purpurate of zinc. A solution of the acetate of zinc produces with the purpurate of ammonia, a solution and precipitate of a beautiful gold yellow colour; and a most brilliant iridescent pellicle, in which green and yellow predominate, forms on the surface of the solution.

Purpurate of tin. A solution of the muriate of tin changes the purpurate of ammonia to a scarlet; but this rapidly disappears, and the solution becomes colourless. After a few hours, white pearly crystals form in abundance, which is the purpurate of tin.

Purpurate of copper. A solution of the acetate or sulphate of copper changes the purpurate of ammonia to a bright yellowish green colour, but produces no precipitation.

Purpurate of nickel. The nitrate of nickel imparts to the purpurate of ammonia a greenish tinge, but produces no precipitation.

Purpurate of cobalt. The acetate of cobalt changes the colour of the same salt to a pale scarlet. After some time, reddish granular crystals form, which are the purpurate of cobalt.

Purpurate of iron. A solution of the green sulphate of iron changes the colour of the purpurate of ammonia to yellowish red, but produces no precipitate.

Such is a very brief account of the *purpurates*, as far as I have examined them. It may at first sight appear singular, that such an insoluble acid should form so many soluble compounds; but when we reflect upon the subject, and consider what a very small quantity of the purpurate of ammonia is retained in solution by water, and that this small quantity has been made the standard of comparison in the above experiments, our surprize is considerably lessened, and we feel no difficulty in conceiving, that if the purpurates were compared with the nitrates, for example, the former would be found by far the least soluble.

From the very small quantities on which I have been obliged to operate, and from other circumstances, I can offer but little respecting the constitution of the purpurates. Those which I have attempted to analyze appear to be anhydrous, and to be composed of two atoms of the acid, and one of the base; and if this be correct, the

same composition may perhaps be referred to most, if not all the compounds above mentioned. The purpuric acid, however, appears capable of forming subsalts and supersalts, with most bases, many of which seem to be very little soluble.

With respect to the characteristic properties of the purpuric acid, I apprehend it may be readily distinguished from all other substances by the beautiful colours exhibited by its alkaline and earthy salts, independently of its other properties, which are likewise peculiar.

The purpuric acid, and its compounds, probably constitute the basis of many animal and vegetable colours. The well known pink sediment, which generally appears in the urine of those labouring under febrile affections, appears to owe its colour chiefly to the purpurate of ammonia, and perhaps occasionally to the purpurate of soda. Some of the purpurates, as for example that of lime, might be probably used as a paint. They might be also used for dying, especially wool and other animal productions.* On this part of the subject, however, as I have little that is certain to offer, I do not deem it prudent to enter at present.

* I may here observe, that the solution of lithic acid in nitric acid, has the property of tinging the skin and other animal substances in a very permanent manner. The colour does not, in general, appear till the substance has been exposed to heat, or what is more effective, to the light of the sun. In the latter case, particularly, a deep purple tint soon makes its appearance, and the substance tinged (more especially the skin) emits during the process a strong and peculiar smell, closely resembling that produced by the nitrate of silver, when applied to the skin, and exposed to similar circumstances.

XXIII. *Astronomical observations and experiments, selected for the purpose of ascertaining the relative distances of clusters of stars, and of investigating how far the power of our telescopes may be expected to reach into space, when directed to ambiguous celestial objects.* By *Sir* William Herschel, *Knt. Guelp. LL.D. F. R. S.*

Read June 11, 1818.

In my last paper on the local arrangement of the celestial bodies in space, I have shown how, by an equalization of the light of stars of different brightness, we may ascertain their relative distances from the observer, in the direction of the line in which they are seen; and from this equalization, a method of turning the space-penetrating power of a telescope into a gradually increasing series of gaging powers has been deduced, by which means the profundity in space, of every object consisting of stars, can be ascertained, as far as the light of the instrument which is used upon this occasion will reach. This method has already been applied to fathom the milky way, and may with equal propriety be used to ascertain the profundity of globular and other clusters of stars in space; I shall therefore make use of some of the numerous observations, contained in my journals and sweeps of the heavens, to show how the distances of these objects may be obtained; and shall also attempt to represent their situation in space by a figure, in which their distances are made proportional to the diameter of a globular space, sufficiently large

to contain all the stars that in the clearest nights are visible to the eye of an observer.

I. *Of the distance of globular and other clusters of stars.*

In observations which are made for ascertaining the distance of a cluster of stars, it is necessary that the gaging power should be marked, which will just make some of the stars belonging to it visible in the telescope that is used for this purpose. If the cluster is of a globular form, but is not insulated, the stars that belong to it may be easily distinguished from those which may happen to be scattered about, or upon it. In clusters of a different construction, the compression, or the apparent size of the stars, must direct the observer.

It is to be remarked, that neither the brightness, nor the diameter of the clusters of which the distance is to be ascertained, are to be considered: some of them are bright enough to be perceived by the eye; others are visible in the finder of the telescope, and many of them can only be seen in the telescope itself. These are circumstances that have no influence on the exactness of the result of the gaging power; but as they regard our knowledge of the construction of these magnificent sidereal systems, an abridged account of them is given, with the observations by which their profundity in space is ascertained; and in the arrangement of these observations, I have followed the order of the space-penetrating power of the instruments by which they were made.

In recording the examination of celestial objects, I have often applied to them the expressions resolvable, or easily resolvable, when, from their appearance, I could not decide

whether they belonged to the class of nebulæ, properly so called, or whether they might not consist of an aggregation of stars, at too great a distance from us to be distinctly perceived; but it is evident that the distance of a cluster of stars cannot be ascertained, as long as it remains doubtful whether the object consists of stars; and that, consequently, their first perceptibility must be the gaging power by which its profundity in space is to be ascertained.

II. *A series of observations of clusters of stars, from which the order of their profundity in space is determined.*

Observation of the 7th cluster of stars in the VIth *class of my catalogues of celestial objects.**

"1784, 20 feet telescope, power 157. An excessively faint "cluster of stars, intermixed with resolvable nebulosity, 8 or "10 minutes in diameter. The stars are so small that they "cannot be seen without the greatest attention; 240 verified "it beyond all doubt. I have suspected many such in this "neighbourhood."

At the time of this observation, the 20 feet telescope was of the NEWTONIAN construction, and its power to penetrate into space was 61.18 times that of the eye, which it has been shown can see stars of the 12th order:† and since it appears from the foregoing observation, that with this power the telescope could but just reach the stars of the cluster, we may conclude that its profundity in space cannot be less than of the 734th order.

* For these catalogues, see Phil. Trans. for 1786, page 471; 1789, page 226, and 1802, p. 503.

† Phil. Trans. for 1817, page 317.

Observation of the 9th cluster in the VIth class.

"1784, 20 feet telescope. A large cluster of exceedingly "small, and compressed stars, about 6 or 7 minutes in "diameter; a great many of the stars are visible, the rest so "small as to appear nebulous; those that are visible are of "one size, and are scattered all over equally. The cluster is "of an irregular round form."

The profundity of the cluster by this observation is of the 784th order.

Observation of the 10th cluster in the VIth class.

"1784, 20 feet telescope. A very compressed, con- "siderably large cluster of the smallest stars imaginable; "all the stars are of a dusky red colour. This cluster is the "next step to an easily resolvable nebula."

The ruddy colour of the stars is probably owing to its low situation; the profundity of the cluster is of the 784th order.

Observation of the 11th cluster in the VIth class.

"1784, 20 feet telescope. A cluster of stars, which, in "respect of the size of the whole as well as the distance and "magnitude of the stars, is a good miniature of the 19th of "the connoissance observed a few minutes ago. The stars, "like those of the foregoing cluster, preserve a faint red "tint. It may be called the next step to an easily resolvable "nebula. It is about $1\frac{1}{2}$ or 2 minutes in diameter."

The profundity of this cluster cannot be much less than of the 784th order. It is in the preceding branch of the milky way.

Observation of the 12*th cluster in the* VIth *class.*

" 1784, 20 feet telescope. This cluster of stars is another " miniature of the 19th of the connoissance, but rather coarser " than my 11th cluster."

The profundity of the 19th of the connoissance being of the 344th order, this cluster, as rather a coarse miniature of it, may be of the 466th order; it is in the preceding branch of the milky way.

Observations of the 17*th cluster in the* VIth *class.*

" 1784, 1785, 20 feet telescope. A very rich cluster of " very compressed and extremely small stars; 4 or 5 " minutes in diameter."

This cluster is probably of a profundity of about the 600th order. It is in the preceding branch of the milky way.

Observations of the 20*th cluster in the* VIth *class.*

" 1785, 1786, 20 feet telescope. Considerably bright, " irregularly round, 8 or 9 minutes in diameter; a great " many of the stars are visible, so that there can remain no " doubt of its being a cluster of the most minute stars " imaginable."

The profundity of this cluster cannot be less than of the 734th order. It is near the south pole of the milky way.

Observation of the 26*th cluster in the* VIth *class.*

" 1786, 20 feet telescope. A very faint cluster of very " compressed extremely small stars; near 4 minutes in " diameter."

The 20 feet telescope being of the construction of the

front view, and having a gaging power of 75.08 gives the profundity of this cluster of the 900th order. It is in the milky way.

Observation of the 35th cluster in the VIth *class.*

" 1788, 20 feet telescope. A small cluster of very faint, " exceedingly compressed stars, about 1 minute in diameter; " the next step to an easily resolvable nebula."

The profundity of this cluster is of the 900th order; it is in the milky way.

Observation of the 38th cluster in the VIth *class.*

" 1791, 20 feet telescope. Considerably bright, small, of " an irregular figure; easily resolvable: some of the stars " are visible."

The profundity of this cluster is of the 900th order. It is in the milky way.

Observation of the 41st cluster in the VIth *class.*

" 1797, 20 feet telescope. Round, resolvable, about 8 " minutes in diameter; very gradually brighter in the " middle. I suppose it to be a cluster of extremely com- " pressed stars; 320 confirms the supposition, and shows a " few of the stars: it must be immensely rich."

The profundity of this cluster is of the 900th order.

Observation of the 63rd cluster in the IVth *class.*

" 1789, 20 feet telescope. Considerably bright, consider- " ably large, irregularly round, very gradually much brighter " in the middle; about 4 minutes in diameter."

The profundity of this cluster must be at least of the 900th order.

Observations of the 1st of the connoissance des temps.

" 1783, 1794, 7 feet telescope. With 287, light without stars."

" 1805, 1809, 10 feet telescope. It is resolvable. There " does not seem any milky nebulosity mixed with what I " take to be small lucid points."

" 1783, 1784, 1809, 20 feet telescope. Very bright, of an " irregular figure; full 5 minutes in the longest direction. I " suspect it to consist of stars."

" 1805, large 10 feet telescope. With 220 the diameter " is 4′ 0″; with this power and light it is what must be " called resolvable."

As all the observations of the large telescopes agree to call this object resolvable, it is probably a cluster of stars at no very great distance beyond their gaging powers; its profundity may therefore be of about the 980th order. It is near the milky way.

Observations of the 2nd of the connoissance.

" 1799, 7 feet finder of the telescope. It is visible as a " star. 1810, it may just be perceived to have rather a " larger diameter than a star."

" 1783, 2 feet sweeper. It is like a telescopic comet."

" 1794, 7 feet telescope. With 287 I can see that it is a " cluster of stars, many of them being visible."

" 1810, 10 feet telescope. A beautiful bright object."

" 1784, 1785, 1802, 20 feet telescope. A cluster of very " compressed exceedingly small stars."

" 1805, 1810, large 10 feet telescope. Its diameter with " 108 is 4′ 59″; with 171 and 220, it is 6′ 0″."

" 1799, 40 feet telescope. A globular cluster of stars."*

By the observation of the 7 feet telescope, which has a power of seeing stars that exceeds the power of the eye to see them 20.25 times, the profundity of this cluster is of the 243rd order.

Observations of the 3rd of the connoissance.

" 1813, 7 feet finder. It is at a small distance from a star " of equal brightness; the star is clear, the object is hazy, " and somewhat larger than the star."

" 1783, 7 feet telescope. With 460 the light is so feeble " that the object can hardly be seen; I suspect some stars in " it. 1813, with 80, many stars are visible in it."

" 1799, 10 feet telescope, power 120; with an aperture of " 4 inches it is resolvable; with 5 easily resolvable; with 6 " it is resolved; with 7 and all open the stars may be easily " perceived."

" 1784, 1785, 20 feet telescope. A beautiful globular " cluster of stars, about 5 or 6 minutes in diameter."

" 1810, Large 10 feet telescope. With 171 the diameter " is full 4′ 30″."

By the observation of the 7 feet telescope this cluster must be of the 243rd order.

Observations of the 4th of the connoissance.

" 1783, 10 feet telescope. All resolved into stars. I can " count a great number of them, while others escape the eye " by their minuteness."

* For the particulars of this observation see Phil. Trans. for 1814, page 274.

" 1783, small 20 feet telescope. All resolved into " stars."

" 1784, 20 feet telescope. The cluster contains a ridge of " stars in the middle, running from south preceding to north " following."

The 10 feet telescope having a power to show stars exceeding that of the eye 28.67 times, gives the profundity of this cluster of the 344th order.

Observations of the 5th of the connoissance.

" 1813, 7 feet finder. It is near a star of equal bright- " ness; the star is clear but the object is hazy."

" 1783, 7 feet telescope. It consists of stars; they are " however so small that I can but just perceive some, and " suspect others. 1810, the globular figure is visible."

" 1783, 10 feet telescope. With 600, all resolved into " stars."

" 1785, 1786, 20 feet telescope. A very compressed " cluster of stars, 7 or 8 minutes in diameter; the greatest " compression about 2 or 2½ minutes."

" 1791, 40 feet telescope. With 370 the stars about the " centre are extremely compressed."

The profundity of this cluster, by the observation of the 7 feet telescope, is of the 243rd order.

Observations of the 9th of the connoissance.

" 1783, 10 feet telescope, power 250. I see several stars " in it; and have no doubt a higher power and more light will " resolve it all into stars."

" 1784, 1786, 20 feet telescope. A cluster of extremely " compressed stars; it is a miniature of the 58d."

By the observations of the 10 feet the profundity is at least of the 344th order. It is in the preceding branch of the milky way.

Observations of the 10th of the connoissance.

" 1783, 7 feet telescope. With 227 I suspected it to consist of stars; with 460 I can see several of them, but they are too small to be counted."

" 1784, 1791, 20 feet telescope. A beautiful cluster of extremely compressed stars; it resembles the 58d; and the most compressed part is about 3 or 4 minutes in diameter."

The profundity of this cluster, by the observation of the 7 feet telescope, is of the 243d order.

Observations of the 11th of the connoissance.

" 1799, 10 feet finder. The cluster is visible; and, directed by neighbouring stars, it may be seen by the eye."

" 1783, 1799, 10 feet telescope. Power 300. With 3 inches " of aperture, the small stars are not to be distinguished; " with 4 inches I can see them."

" 1803, 1810, large 10 feet telescope. The cluster is of " an irregular form, from 9 to 12 minutes in diameter."

The 10 feet telescope with an aperture of 4 inches, had a gaging power of 12.02; the profundity of this cluster is therefore of the 144th order. It is in the milky way.

Observations of the 12th of the connoissance.

" 1799, 10 feet finder. The object is visible in it."

" 1783, 1799, 10 feet telescope. With 120, and an aper-
" ture of 4 inches, easily resolvable; with 5 inches, stars
" become visible; with 6 inches, pretty distinctly visible; and
" with all open, the lowest power shows the stars."

" 1785, 1786, 20 feet telescope. A brilliant cluster, 7 or
" 8 minutes in diameter; the most compressed parts about 2
" minutes."

With an aperture of 5 inches the 10 feet telescope had a gaging power of 15.53; and this cluster is consequently of a profundity of the 186th order.

Observations of the 13*th of the connoissance.*

" 1799, 1805. It is very plainly to be seen by the eye."

" 1799, 7 feet finder. Very visible."

" 1783, 7 feet telescope. With 227 plainly resolved into
" stars."

" 1799, 10 feet telescope. With an aperture of 4 inches
" the stars cannot be distinguished; with 9 inches, very
" beautiful."

" 1787, 1799, 20 feet telescope. The stars belonging to
" the cluster extend to 8 or 9 minutes in diameter; the most
" compressed part about 2 or $2\frac{1}{2}$; the latter is round, the
" former irregular."

" 1805, large 10 feet telescope. A brilliant cluster all
" resolved into stars."

By the observation of the 7 feet telescope, the profundity of this cluster is nearly of the 243d order.

Observations of the 14th of the connoissance.

1783, 7 feet telescope. With 227, there is a strong suspicion of its consisting of stars.

" 1783, 1784, 1791, 1799, 20 feet telescope. Extremely " bright, round, easily resolvable; with 300 I can see the " stars. The heavens are pretty rich in stars of a certain " size, but they are larger than those in the cluster, and " easily to be distinguished from them. The cluster is consi- " derably behind the scattered stars, as some of them are " projected upon it."

From the observations of the 20 feet telescope, which in 1791 and 1799 had the power of discerning stars 75.08 times as far as the eye, the profundity of this cluster must be of the 900th order.

Observations of the 15th of the connoissance.

" 1799. It is visible to the eye."

" 1783, 1794, 7 feet telescope. With 278 the stars of the " cluster may be seen."

" 1799, 10 feet telescope. With an aperture of 4 inches, " no trace of stars is visible. 1817, with an aperture of 4.56 " inches, which gives a gaging power of 14, it appears like a " nebulous patch, gradually brighter in the middle; with a " gaging power of 16, the hazy border of it is larger; with " 18, the whole of it much larger and brighter; with 20, " resolvable; and with 22, the stars are visible."

" 1784, 1787, 1807, 20 feet telescope. A globular cluster " of stars, about 6 minutes in diameter."

" 1810, large 10 feet telescope. The diameter, with 171,

" is full 4′ 30″, and taking in the stars that probably belong " to it, it is 6′ 45″."

By the observation of the 7 feet telescope, the profundity of this cluster is of the 243d order.

Observations of the 19*th of the connoissance.*

" 1783, 10 feet telescope. With 250, I can see 5 or 6 " stars, and all the rest appears mottled like other objects of " this kind, when not sufficiently magnified or illuminated."

" 1784, 20 feet telescope. A cluster of very compressed " stars, much accumulated in the middle; 4 or 5 minutes in " diameter."

By the observation of the 10 feet telescope, the profundity of this cluster is of the 344th order. It is in the preceding branch of the milky way.

Observations of the 22*d of the connoissance.*

" 1783, 7 feet telescope. 460 has not light enough to show " it; with 227, I see it very imperfectly."

" 1801, 10 feet telescope. With 600 it is a cluster of stars."

" 1783, small 20 feet telescope. With 350, all resolved " into stars."

" 1784, 20 feet telescope. An extensive cluster of stars."

" 1810, large 10 feet telescope. The stars are condensed " in the middle. The diameter is 8′ 0″; the greatest conden- " sation is about 4′ 0″."

By the observation of the 10 feet telescope, the profundity of this cluster must be nearly of the 344th order. It is near the following branch of the milky way.

Observations of the 30th of the connoissance.

" 1794, 7 feet finder. It is but just visible."

" 1794, 7 feet telescope. It seems to be resolvable, but is " too faint to bear a high power."

" 1810, 10 feet telescope. With 71, it appears like a " pretty large cometic nebula, very gradually much brighter " in the middle. 1783, with 250 it is resolved into very " small stars."

" 1783, small 20 feet NEWTONIAN, 12 inch diameter. Power " 200; it consists of very small stars; with two rows of stars, " 4 or 5 in a line."

" 1783, large 20 feet NEWTONIAN. Power 120; by a " drawing of the cluster, the rows of stars probably do not " belong to the cluster."

" 1784, 1785, 1786, 20 feet telescope, power 157. A " brilliant cluster."

" 1810, large 10 feet telescope. With 171 and 220 the " diameter is 3′ 5″; it is not round."

By the observation of the 10 feet telescope, the profundity of this cluster is of the 344th order.

Observations of the 33d of the connoissance.

" 1799, 10 feet finder. It is visible as a faint nebula."

" 1783, 1794, 7 feet telescope. With 75, it has a nebulous " appearance; it will not bear 278 and 460, but with 120 it " seems to be composed of stars."

" 1799, 1810, 10 feet telescope. The brightest part is " resolvable; some of the stars are visible."

" 1805, 1810, Large 10 feet telescope. The condensation

" of the stars is very gradual towards the middle; but with " the four powers 71, 108, 171, and 220, some nebulosity " remains. The stars of the cluster are the smallest points " imaginable. The diameter is nearly 18 minutes."

The profundity of this cluster, by the observation of the 10 feet telescope, must be of the 344th order.

Observations of the 84th of the connoissance.

" 1799, 7 feet finder. It is visible."

" 1783, 1794, 7 feet telescope. A cluster of stars; with " 120, I think it is accompanied with mottled light, like stars " at a distance."

" 1784, 1786, 20 feet telescope. A coarse cluster of large " stars of different sizes."

By the observation of the 7 feet telescope, the profundity of this cluster does probably not exceed the 144th order.

Observations of the 85th of the connoissance.

" 1794, It is visible to the naked eye as a very small " cloudiness."

" 1783, 1794, 1801, 1813, 7 feet telescope. It is a rich " cluster of stars of various sizes."

" 1806, 10 feet telescope. There is no central contraction " to denote a globular form."

" 1783, 1785, 20 feet telescope. A cluster of pretty com- " pressed large stars."

The profundity of this cluster does probably not exceed the 144th order. It is in the milky way.

Observations of the 53d of the connoissance.

" 1813, 7 feet finder. It appears like a very small " haziness."

" 1783, 7 feet telescope. With 460 the object is ex- " tremely faint. 1813, with 118 it is easily resolvable, and " some of the stars may be seen."

" 1783, 10 feet telescope. With 250, I perceive 4 or 5 " places that seem to consist of very small stars."

" 1784, 1786, 20 feet telescope. A globular cluster of " very compressed stars."

From the observation of the 7 feet telescope, it appears that the profundity of this cluster is of the 243d order.

Observations of the 55th of the connoissance.

" 1783, small 20 feet telescope. With 250 fairly resolved " into stars; I can count a great many of them, while others " are too close to be distinguished separately."

" 1784, 1785, 20 feet telescope. A rich cluster of very " compressed stars, irregularly round, about 8 minutes " long."

By the observation of the small 20 feet telescope, which could reach stars 38.99 times as far as the eye, the profundity of this cluster cannot be much less than of the 467th order: I have taken it to be of the 400th.

Observations of the 56th of the connoissance.

" 1783, 7 feet telescope. A strong suspicion of its being " stars."

" 1783, 1799, 10 feet telescope. 120 will not resolve it;

" 240 wants light: 350 however shows the stars, but they " are so exceedingly close and small that they cannot be " counted."

" 1784, 1807, 20 feet telescope. A globular cluster of " very compressed small stars about 4 or 5 minutes in dia- " meter."

" 1805, 1807, large 10 feet telescope. With 171 it is 3′ 36″ " in diameter."

The profundity of this cluster, by the observation of the 10 feet telescope, must be of the 344th order. It is near the preceding branch of the milky way.

Observations of the 57th of the connoissance.

" 1782, 7 feet telescope. I suspect it to consist of very " small stars; in the middle it seems to be dark."

" 1783, 1805, 1806, 10 feet telescope. With 130 it seems " to be a rim of stars, but with 350 there remains a doubt. " It is a little oval; the dark place in the middle is also oval; " one side of the bright margin is a little narrower than the " other."

" 1784, 1799, 20 feet telescope. It is an oval with a dark " place within; the light is resolvable. 240 showed several " small stars near, but none that seem to belong to it. It is " near 2 minutes in diameter."

" 1805, large 10 feet telescope. By a meridian passage of " 7 seconds of sidereal time, the diameter is 1′ 28″.4."

By the observation of the 20 feet telescope, the profundity of the stars of which it probably consists must be of a higher than the 900th order; perhaps 950.

Observations of the 62d of the connoissance.

" 1783, 10 feet telescope. With 250, a strong suspicion, " amounting almost to a certainty, of its consisting of stars."

" 1785, 1786, 20 feet telescope. Extremely bright, round, " very gradually brighter in the middle, about 4 or 5 minutes " in diameter. 240 with strong attention showed the stars " of it. The cluster is a miniature of the 3d of the con- " noissance."

By the 20 feet telescope, which at the time of these observations was of the NEWTONIAN construction, the profundity of this cluster is of the 734th order. It is in the preceding branch of the milky way.

Observations of the 67th of the connoissance.

" 1783, 7 feet telescope. A cluster of stars."

" 1809, 10 feet telescope. A cluster of very small stars."

" 1784, 20 feet telescope. A most beautiful cluster of " stars; not less than 200 in view."

By estimation, the profundity of this cluster may be of the 144th order.

Observations of the 68th of the connoissance.

" 1786, 1789, 1790, 20 feet telescope. A cluster of very " compressed small stars, about 3 minutes broad and 4 " minutes long. The stars are so compressed, that most of " them are blended together."

Probably the stars of this cluster might be perceived by a 10 feet telescope, so that its profundity may be of the 344th order.

Observation of the 69th of the connoissance.

" 1784, 20 feet telescope. Very bright, pretty large, easily " resolvable, or rather an already resolved cluster of minute " stars. It is a miniature of the 53d of the connoissance."

By this observation, the profundity of the cluster must be of the 734th order.

Observations of the 71st of the connoissance.

" 1794, 7 feet telescope. With 120 and 160 the stars of it " become just visible."

" 1783, 1799, 1810, 10 feet telescope. A cluster of stars " of an irregular figure."

" 1784, 1799, 1807, 20 feet telescope. It is situated in the " milky way, and the stars are probably in the extent of it; " it is however considerably condensed; about 3 minutes in " diameter."

" 1805, large 10 feet telescope. An irregular cluster of " very small stars, 2′ 35″ in diameter."

By the observation of the 7 feet telescope, the profundity of this cluster is of the 243d order. It is in the following branch of the milky way.

Observations of the 72d of the connoissance.

" 1805, 7 feet telescope. With a power of 80 the stars " may just be perceived."

" 1783, 1810, 10 feet telescope. With 150 fairly re- " solved."

" 1784, 1788, 20 feet telescope. A cluster of very small " stars."

" 1810, large 10 feet telescope. A globular cluster; its " diameter is 2′ 40″."

" 1810, 40 feet telescope. A beautiful cluster of stars."

By the observation of the 7 feet telescope, the profundity of this cluster must be of the 243d order.

Observations of the 74th of the connoissance.

" 1783, 1784, 7 feet telescope. With 100 and 120 it is a " collection of very small stars; I see many of them."

" 1799, 1801, 10 feet telescope. Several of the stars are " visible; it is a very faint object."

" 1784, 20 feet telescope. Some stars are visible in it; " the edges are not resolvable."

" 1805, 1810, large 10 feet telescope. With 108 it con- " sists of extremely small stars, of an irregular figure; a " very faint object of nearly 12 minutes in diameter."

" 1799, 40 feet telescope. Very bright in the middle, but " the brightness is confined to a very small part."

By the observation of the 7 feet telescope, the profundity of the nearest part of this cluster must be of the 243d order, but most probably a succession of more distant stars was seen in the larger telescopes.

Observations of the 75th of the connoissance.

" 1799, 7 feet finder. It is but just visible."

" 1799, 7 feet telescope. There is not the least appearance " of its consisting of stars, but it resembles other clusters of " this kind, when they are seen with low space-penetrating " and magnifying powers."

"1810, 10 feet telescope. With 71 it is small and "cometic."

"1784, 1785, 20 feet NEWTONIAN. Easily resolvable; "some of the stars are visible."

"1810, 20 feet front view. It is a globular cluster."

"1799, 1810, large 10 feet. Its diameter with 171 is "1′ 48″; with 220 it is 2′ 0″."

By the observation of the 20 feet NEWTONIAN telescope, the profundity of this cluster must be of the 734th order.

Observations of the 77th of the connoissance.

"1783, 7 feet telescope. An ill defined star, surrounded "by nebulosity."

"1801, 1805, 1809, 1810, 10 feet telescope. It has "almost the appearance of a large stellar nebula."

"1783, 1785, 1786, 20 feet telescope. Very bright; an "irregular extended nucleus with milky chevelure, 3 or 4 "minutes long, near 3 minutes broad."

"1801, 1805, 1807, large 10 feet telescope. A kind of "much magnified stellar cluster; it contains some bright "stars in the centre. With 171 its diameter is 1′ 17″; with "220 it is 1′ 36″."

From the observations of the large 10 feet telescope, which has a gaging power of 75.82, we may conclude that the profundity of the nearest part of this object is at least of the 910th order.

Observations of the 79th of the connoissance.

"1783, 7 feet telescope. With 57 nebulous; with 86 a "strong suspicion of its being stars."

" 1799, 10 feet telescope. 300 shows the stars of it with " difficulty."

" 1784, 20 feet telescope. A beautiful cluster of stars, " nearly 3 minutes in diameter."

" 1806, large 10 feet telescope. A globular cluster, the " stars of which are extremely compressed in the middle; " with 171 and 220 the diameter is 2′ 50″, but the lowness of " the situation probably prevents my seeing the whole of its " extent."

By the observation of the 10 feet telescope the profundity of the cluster is of the 344th order.

Observations of the 80th of the connoissance.

" 1784, 1786, 20 feet telescope. A globular cluster of " extremely minute and very compressed stars of about 3 or " 4 minutes in diameter; very gradually much brighter in " the middle; towards the circumference the stars are " distinctly to be seen, and are the smallest imaginable."

The profundity of this cluster is probably not much less than of the 734th order.

Observations of the 92d of the connoissance.

" 1799, 7 feet finder. It may just be distinguished; it is " but very little larger than a star."

" 1783, 2 feet sweeper. With 15 it appears like a clouded " star."

" 1783, 7 feet telescope. With 227 resolved into very " small stars; with 460 I can count many of them."

" 1799, 10 feet telescope. With 240 the stars are much " condensed in the centre."

" 1783, 1787, 1799, 20 feet telescope. A brilliant cluster;
" about 6 or 7 minutes in diameter."

" 1805, large 10 feet telescope. The most condensed part
" is 3′ 16″ in diameter."

The profundity of this cluster, by the observation of the 7 feet telescope, is of the 243d order.

Observations of the 97th of the connoissance.

" 1799, 7 feet finder. The object is not visible in it."

" 1789, 20 feet telescope; considerably bright, globular,
" of equal light throughout, with a diminishing border of no
" great extent. About 3 minutes in diameter."

" 1805, large 10 feet telescope. The constellation being
" too low it had the appearance of a faint nebula."

From the observation of the 20 feet telescope, it appears that the profundity of this object is beyond the gaging power of that instrument; and as it must be sufficiently distant to be ambiguous, it cannot well be less than of the 980th order.

III. *Of a method to represent the profundity of celestial objects in space by a diagram.*

In order to represent the profundity of celestial objects in space, I shall have recourse to the construction of an astronomical globe, on the surface of which the situations of the heavenly bodies are pointed out to us in the given two dimensions of right ascension and polar distance; but as their distance from an eye placed in the centre of the globe cannot be expressed by their situation on the surface, I shall endeavour to show that this deficiency may be artificially

supplied in a figure representing such a globe, by the addition of lines that are of a length which is proportional to the diameter of it.

It has been shown in my last paper, that all the stars which may be seen in the clearest nights, are probably contained within a globular space, of which the radius does not exceed the 12th order of distances; I shall therefore suppose the circle c in the centre of fig. 1, [Plate XXI.] to represent a celestial globe, containing all the stars that are generally marked on its surface; their arrangement within this globular space, however, must be supposed to be according to their order of distances, the stars of the first order being placed nearest the centre, and those of the 2d, 3d, and 4th, &c. gradually farther off; but they must all be placed in their well ascertained directions, so that a line from the centre drawn through any one of them may come to the surface at the place where its situation is marked.

According to this assumption it follows, that all those celestial objects which are farther than the 12th order of distances from the centre, must be represented as being at the outside of the globular space; but as our celestial globes represent not only the situation of the stars of the heavens, but give us also many additional objects, such as clusters of stars, nebulæ, and the milky way, it is evident that the point where the line of sight from the centre to any one of these distant objects, leaves the surface of the globular space, is ascertained; and since any celestial object not inserted on our globes, of which the right ascension and polar distance are given, may be easily added, the position of the visual ray directed to such an object will thereby also be determined.

In my last paper I have drawn the attention of astronomers to the condition of the milky way, as being the most brilliant, and beyond all comparison the most extensive sidereal system; and have also shown that the globular space containing all our visible stars, is situated within its compass; I shall therefore now make the plane of it the principal dimension of my figure; then if the line *a b* represent this plane, a perpendicular drawn from the centre *c* of the figure to *d* and to *e*, will be directed towards the north and south poles of it, and the situation of the globular space in the figure will be like that of a celestial globe adjusted to the latitude of 90 degrees, having the milky way in the horizon, the 190th degree of right ascension in the meridian, and the 60th degree of north polar distance in the zenith.

From this description of the arrangement of the stars within the globular space, and its situation in the plane of the milky way, it is evident that, having already an expression for the position of a celestial object in two dimensions, the addition of the third, which is its profundity or central distance, may be represented by a line of a length that is proportional to the diameter of the globular space; and if this line be a continuation of the direction in which the object is seen from the centre, its termination will show the real place of the object, and point out its situation with respect to the great sidereal stratum of the milky way.

An observer who looks at a celestial globe, and wishes to see the angle of the direction of the line in which an object is seen from the centre, will for this purpose turn the globe horizontally till the plane of the azimuth circle is at right angles to the line in which he looks at it; or, if more con-

venient, he will change his position by going round the globe till he comes to the situation in which this angle will appear of its true magnitude.

In illustration of this, let NESW, figure 2, [Pl. XXI.] be the circle on which the azimuths of celestial objects are to be reckoned, and let the meridional line NS pass through the 190th degree of right ascension at S; then will the numbers at the circumference of the circle point out the degrees, and the quadrant of the azimuth of the situation in which any object is to be seen when referred to the milky way. The particular use of this azimuth circle will appear, when the construction of the figure which expresses the profundity of the clusters of stars, of which I have given the observations, has been explained.

Having fixed upon the plane of the milky way as the region of the heavens to which the situation of the clusters of stars is to be referred, their right ascension and polar distance, which are required for this purpose, must be reduced to this plane; and will appear under the denomination of elevation and azimuth. The elevation from the plane of the milky way will be either north or south, and the azimuths in either the northern or southern hemisphere of it, will be in the north-east, south-east, north-west, and south-west quadrants. In order to make this reduction, we have the construction of the triangle ABC, figure 3, in which A is the pole of the heavens; B the north pole of the milky way, and C the situation of the cluster of stars; and there is given the side AB, which is the distance of the two poles; the side AC, which is the polar distance of the cluster, and the angle A, which is the difference between the right ascension of the

pole of the milky way and that of the cluster of stars. From these data we find the side BC, the complement of which is the angle of the elevation of the cluster; and the angle ABC, or its supplement CBD, which gives the degree and the quadrant of the azimuth of the cluster. When to these two particulars the profundity of a cluster is added, we have its local situation, with regard to the plane of the milky way, in the required three dimensions of space.

The following table is the result of a set of calculations made for the purpose of obtaining the above mentioned particulars.

Clusters of stars taken from my catalogues.				
Class No.	Profundity.	Elevation.	Azimuth.	Point of sight.
VI. 7	734	76° 58′ N	31° 43′ SE	58° 17′ SW
9	734	73 25 N	87 2 SE	2 58 SW
10	734	14 11 N	48 42 SE	41 18 SW
11	734	8 35 N	55 10 SE	34 50 SW
12	460	6 20 N	54 30 SE	35 30 SW
17	600	2 52 N	63 49 NW	26 11 SW
20	734	87 39 S	10 57 NW	79 3 SW
26	900	0 5 S	35 38 NW	54 22 SW
35	900	0 27 N	1 55 NE	88 5 SE
38	900	4 31 S	77 5 NE	12 55 SE
41	900	32 55 N	16 9 NE	73 51 SE
IV. 63	900	59 47 N	10 23 NE	79 37 SE
Clusters of stars taken from the connoissance.				
1	980	4° 42′ S	61° 24′ NW	28° 36′ SW
2	243	35 29 S	68 17 NE	21 43 SE
3	243	78 29 N	89 38 NE	0 22 SE
4	344	14 31 N	47 41 SE	42 19 SW
5	243	45 36 N	59 27 SE	30 33 SW
9	344	9 35 N	62 19 SE	27 41 SW
10	243	22 11 N	71 28 SE	18 32 SW
11	144	3 10 S	84 21 SE	5 39 SW
12	180	25 26 N	71 57 SE	18 3 SW
13	243	41 19 N	65 31 NE	24 29 SE
14	900	14 6 N	77 48 SE	12 12 SW
15	243	26 38 S	57 3 NE	32 57 SE
19	344	7 56 N	53 51 SE	36 9 SW
22	344	8 45 S	67 2 SE	22 58 SW
30	344	47 26 S	86 5 SE	3 55 SW
33	344	29 25 S	10 37 NW	79 23 SW
34	144	13 48 S	20 33 NW	69 27 SW
35	144	3 13 N	63 58 NW	26 2 SW
53	243	77 58 N	28 6 SE	61 54 SW
55	400	24 19 S	66 30 SE	23 30 SW
56	344	8 59 N	60 43 NE	29 17 SE
57	950	16 51 N	61 28 NE	28 32 SE
62	734	5 54 N	50 29 SE	39 31 SW
67	144	31 44 N	83 4 SW	6 56 SE
68	344	34 19 N	3 1 SW	86 59 SE
69	734	11 35 S	59 6 SE	30 54 SW
71	243	4 10 S	86 6 NE	23 54 SE
72	243	32 58 S	86 40 NE	3 20 SE
74	243	43 53 S	15 28 NW	74 32 SW
75	734	26 29 S	78 9 SE	11 51 SW
77	910	50 32 S	47 36 NW	42 24 SW
79	344	29 25 S	76 47 SW	13 13 SE
80	734	18 41 N	48 39 SE	41 21 SW
92	243	35 33 N	55 50 NE	34 10 SE
97	980	58 52 N	26 5 NW	63 55 SW

In order to explain the construction of the table, and the use that is to be made of it when the situation of any one of the clusters delineated in figure 1 is to be examined, I shall take the first cluster it contains for an example.

The first column points out the class and number, where the clusters taken from my catalogues are to be found, and only the number of those that are taken from the Connoissance des Temps for 1784. In the figure, the place of the cluster whose situation is to be examined is distinguished by the same mark as in the table namely VI, 7.

The second column contains the distance of the same cluster from an eye placed in the centre of the globular space, the profundity of which is 734, as determined by the observations that have been given. In the figure it is expressed by the length of the line *c* VI, 7 drawn from the centre of it to the cluster, whose length is 734, the radius of the circle representing the globular space being 12.

The third column gives the angle of elevation of the cluster, which in the present instance is 76° 58′ above the northern plane of the milky way. In the figure it is expressed by the central meeting of the lines *b c* and *c* VI, 7: one of which denotes this plane, and the other the profundity of the cluster.

To find the quantity of this angle, it is necessary to have the right ascension and polar distance of the cluster; and here it will be proper to notice that I have deduced these requisites from my own observations of the clusters, brought to the beginning of the year 1800. Then to find the elevation of the present cluster by the method which has been explained, we have in figure 3, the side AB = 60°: the side

AC = 71° 17′, being the polar distance of the cluster; and the angle A = 6° 56′ 45″, being the right ascension of the cluster 196° 56′ 45″ minus 190°. By these quantities we find BC = 13° 2′ 28″, and its complement 76° 57′ 32″, which is the required elevation of the cluster VI, 7.

The fourth column assigns the azimuth of the cluster; and as the degrees of the quadrants of the azimuth circle in figure 2, are numbered one from the south the other from the north, the letter S is prefixed to E, to show that the degrees of it are to be looked for in the south-east quadrant; the quantity of the angle, in consequence of the foregoing calculation, is easily obtained; for as we now already have the side BC, the opposite angle A, and the side AC, we find the supplemental angle CBD, which gives the azimuth 31° 43′ 9″. By this result the situation of the direction, in which an observer in the centre of the globular space must look to see the cluster, is determined.

The fifth column contains the point of sight, or situation in which the eye of an observer should be placed, when, by the assistance of a celestial globe, the profundity of any cluster marked in the figure is to be examined. This point, for the cluster VI, 7, is 58° 17′ south-west, which denotes that the globe must be turned horizontally till the 58th degree of the south-west quadrant directly faces the observer, or that, by changing his situation, he must place himself so as to face the globe in the assigned position.

I have called the construction of the figure which gives the profundity of the clusters, an artificial one; because, as soon as the celestial globe is brought into the situation where it can be seen from the tabular point of sight, the figure will

always be found already prepared to show by inspection the azimuth, the elevation and the profundity of the cluster under examination; for as the globe, which in its adjusted situation has the azimuth of the cluster VI, 7, at right angles to the line of sight, so the globular space in the centre of the figure being supposed similarly arranged, has the tabular azimuth 31° 43′ SE also at right angles to the line drawn to the figure, when seen from the point of sight 58° 17′ SW.

The direction from the centre of the globe to the place on its surface where the cluster is inserted, is also preserved in the continuation of it beyond the surface of the globular space, by the angle of its elevation 76° 58′ above the northern plane of the milky way.

The profundity of the cluster, as has already been noticed, is expressed by the continuation of the line of elevation to 784 such parts as the radius of the globular space contains 12; and it may not be amiss, by way of assisting our conception of the vast distance of the situation at which this cluster is placed, to state, that if a line directed to it were added to an 18 inch globe, supposed to contain all the visible stars of the heavens, its length to express this distance would be above 45 feet.

This figure which, from its construction, represents all the different aspects in which a celestial globe should be seen, when its horizontal position for any cluster is adjusted by the foregoing table, has the imperfection that, on account of the different azimuths of their situation, they cannot all be collected into one perspective view; but as it affords the means of examining them separately, which may even be done without the assistance of the globe, this inconvenience

is compensated by the advantage it has of showing all the angles of elevation, and the comparative lengths of the lines expressing the profundity of the clusters in their true magnitude, which an orthographic projection of their situation could not have done.

IV. *Of ambiguous celestial objects.*

When the nature or construction of a celestial object is called ambiguous, this expression may be looked upon as referring either to the eye of the observer, or to the telescope by which it has been examined. In the foregoing observations we find that the 11th, 13th, 15th, and 35th of the connoissance, when they are at a sufficient altitude for the purpose, may be seen by the eye; but as, without artificial vision, they appear only under the semblance of very small, faint cloudy spots, we should not be able to decide whether they were of a nebulous or sidereal condition, if we were not informed by the telescope that they are brilliant clusters of stars; the eye therefore sees them as ambiguous objects.

If these objects are ambiguous when only viewed by the unassisted eye, there are others that will appear to be so, when they are seen through such small telescopes as are generally attached to large ones, and are called finders, because they point out objects that are not visible to the eye. With regard to these finders, I have occasionally used them of different sizes and constructions; but from experience I can say, that a small one of a most simple composition, with a power of penetrating into space of about four times that of the eye, has generally been sufficient for all the purposes of a 7 or 10 feet telescope; because these instruments may

easily be made to act as finders to themselves, by using a double eye glass with a large field of view and a small magnifying power. It is indeed very obvious that when a small telescope, acting as a finder to a larger one, has not sufficient light to show the objects we look for, a more powerful one must be used. In this manner I have often been obliged to have recourse to a 10 feet reflector as a finder, to point out the situation of an object to be viewed in the 20 feet telescope.

It may have appeared singular, that among the observations which have been given, there are many that were made by the 7 and 10 feet telescope finders, but the important use of these observations will appear in the consequences that may be drawn from them; for the clusters of stars, No. 2, 3, 5, 12, 30, 33, 34, 53, 75, and 92 of the connoissance were all to be seen in these finders; they were, however, not seen as clusters of stars, but as ambiguous objects. No. 12, 30, 34, and 75 were but just to be perceived; No. 2 and 92 appeared like stars with rather a large diameter; No. 3 and 5 like hazy stars; No. 33 and 53 like small hazinesses or nebulosities; and yet they were all proved by the telescopes in which they were critically examined to be clusters of stars. If then a cluster of stars in a very small telescope will appear like a star with rather a larger diameter than stars of the same size generally have, we shall certainly be authorised to conclude, that an object seen in a larger and more perfect telescope as a star with rather a larger diameter, is also an ambiguous object, and might possibly be proved to be a cluster of stars, had we a superior instrument by which we could examine its nature and construction.

This seems to throw some light upon a species of objects called stellar nebulæ, one hundred and forty of which have been inserted in my catalogues. For as it has just been mentioned that a 10 feet telescope may become a finder to a 20 feet one, the 20 feet telescope itself will be but a finder to objects that are so far out of its reach as not to appear otherwise than ambiguous; nay, the 40 feet telescope, when it is but just powerful enough to show the existence of an object which decidedly differs from the appearance of a star, may then truly be called a finder.

V. *The milky way, at the profundity beyond which the gaging powers of our instruments cannot reach, is not an ambiguous object.*

Celestial objects can only be said to remain ambiguous, when the telescopes that have been directed to them leave it undetermined whether they are composed of stars or of nebulous matter. Six observations of different parts of the milky way, relating to this subject, have already been given in my last paper,* to which the following four may be added.

Dec. 27, 1786. Right ascension 6^h 42′. Polar distance 88° 38′. There are 116 stars in the field of view, besides many too small for the gage.

Sept. 21, 1788. Right ascension 21^h 29′. Polar distance 41° 1′. There are about 360 stars in the field of view, but most of them are so small that it requires the utmost attention to see them.

Sept. 27, 1788. Right ascension 21^h 17′. Polar distance

* See Phil. Trans. for 1817, pages 325, 326, and 329.

52° 50′. With 157 there are small stars with suspected nebulosity; 300 shows a great many smaller stars intermixed with the former.

Sept. 11, 1790. Right ascension 19h 50′. Polar distance 47° 0′. About 240 stars in the field of view, with many too small to be counted.

In these ten observations the gages applied to the milky way were found to be arrested in their progress by the extreme smallness and faintness of the stars; this can however leave no doubt of the progressive extent of the starry regions; for when in one of the observations a faint nebulosity was suspected, the application of a higher magnifying power evinced, that the doubtful appearance was owing to an intermixture of many stars that were too minute to be distinctly perceived with the lower power; hence we may conclude, that when our gages will no longer resolve the milky way into stars, it is not because its nature is ambiguous, but because it is fathomless.

VI. *Of the assumed semblance of clusters of stars, when seen through telescopes that have not light and power sufficient to show their nature and construction.*

The variety of telescopes used in the long series of observations that have been given, will afford us many instances to ascertain the various deceptive appearances that clusters of stars may put on when they are observed with an inadequate apparatus.

An examination of some particulars relating to this subject may assist us to ascertain in what class we ought probably to

place the numerous observations of ambiguous objects that in my sweeps of the heavens were seen by the 20 feet telescope; and having already compared the different forms under which clusters of stars appeared in the finders of the instruments, I shall now also notice how they were seen in the gradually larger telescopes.

In the 2 feet NEWTONIAN sweeper,

No. 92 of the connoissance appeared like a clouded star, with a magnifying power of 15. No. 2, with a power of 24, appeared like a telescopic comet.

In the 7 feet telescope,

No. 77 was like an ill defined star, surrounded by nebulosity. No 79, with a power of 57, appeared nebulous. With 460 No. 3 could hardly be seen, for want of light. No. 10, with 227, could not be resolved into stars, for want of power. With 460 No. 22 wanted light, and with 227 it wanted power. With a magnifier of 171 No. 33 had a nebulous appearance. No. 1 was seen as light without stars.

In the 10 feet telescope,

The light of No. 19 appeared mottled. With a power of 71 No. 30 appeared like a pretty large cometic nebula, very gradually much brighter in the middle. With the same power No. 75, was small and cometic. No. 77 had nearly the appearance of a large stellar nebula.

In the large 10 feet telescope,

No. 97, being too low for examination, had the appearance of a faint nebula.

The numerous ambiguous objects that have been seen in the 20 feet telescope do not properly come under this head; for as none of them have been critically examined by superior telescopes, they must still remain ambiguous; and it is for the purpose of being able to form some probable conjecture about the nature of these doubtful objects, that the foregoing results of the appearance of such as have been ascertained to be clusters of stars, have been pointed out.

It would be far too extensive to enter into particular applications, I shall therefore confine myself to a few general remarks. In the depth of the celestial regions, we have hitherto only been acquainted with two different principles, the nebulous and the sidereal. The light of the nebulous matter is comparatively very faint, and, except in a few instances, invisible to the eye. It is also in general widely diffused over a great expanse of space, in which, by an increase of faintness, it generally escapes the sight: the light of stars on the contrary, is comparatively very brilliant, and confined to a small point, except when many of them are collected together in clusters, when their united lustre sometimes takes p a considerable number of minutes of space; but in this case the stars of them may be seen in our telescopes, and by the observations that have been given, it appears that when they are viewed with instruments gradually inferior to those which prove them to be clusters of stars, their diameters, seen with less light and a smaller magnifying power, are

generally contracted; a globular cluster is reduced to a cometic appearance; to an ill defined star surrounded by nebulosity, and to a mere small star with rather a larger diameter than stars of the same size generally have. In consequence of these considerations, it seems to be highly probable that some of the cometic, many of the planetary, and a considerable number of the stellar nebulæ, are clusters of stars in disguise, on account of their being so deeply immersed in space, that none of the gaging powers of our telescopes have hitherto been able to reach them. The distance of objects of the same appearances, but which are of a nebulous origin, on the contrary, must be so much less than that of the former, that their profundity in space may probably not exceed the 900th order.

VII. *Of the extent of the power of our telescopes to reach into space, when they are directed to ambiguous celestial objects.*

The method of equalising the light of stars on which the gaging power of telescopes has been established, may also be applied to give us an estimate of the extent of their power to reach ambiguous celestial objects.

When the united light of a cluster of stars is visible to the eye, there will then be a certain maximum of distance to which the same cluster might be removed so as still to remain visible in a telescope of a given space-penetrating power; and if the distance of this cluster can be ascertained by the gaging power of any instrument that will just show the stars of it, the order of the profundity at which the cluster could still be seen as an ambiguous object, may be ascertained

by the space-penetrating power of the telescope through which it is observed. But as the aggregate brightness of the stars depends entirely on their number and arrangement, this method can only be used with clusters of stars that have been actually observed.

The 35th of the connoissance, for instance, being visible to the eye as a small cloudiness, its profundity in space was, by an observation of the 7 feet telescope, shown probably not to exceed the 144th order; then, as the stars that enter into the composition of this cluster are of such an arrangement that their united lustre may be seen by the eye at the distance of the 144th order, the 10 feet telescope, by which this cluster was viewed, having a power of penetrating into space 28.67 times that of the eye, would be able to show this cluster as a small cloudiness, if it were removed to the distance of the 4128th order. The 20 feet NEWTONIAN telescope, in which it was also observed, having a space-penetrating power 61.16 times that of the eye, would still be sufficient to discover it as an ambiguous object, if it were removed to the distance of the 8809th order.

To investigate how far the 15th cluster, which is also visible to the eye, might be removed, so as still to be seen in the front view of the 20 feet telescope, we find, by inspecting the table in which the profundities are given, that the eye can reach it at the distance of the 243d order; therefore this telescope, with a power 75.08 times that of the eye, would still be able to show it at the distance of the 18244th order, and being a globular cluster, its appearance would be that of a small star with rather a large diameter.

As there are but few clusters of stars that can be seen by the eye, the observations of their visibility in the finders of telescopes, and their appearance in them, are of eminent use in ascertaining the distance at which we can expect to see celestial objects in large telescopes; when, therefore, a cluster of stars cannot be seen by the eye, its visibility in the finder must first of all be reduced to the standard of the eye. I have already noticed that the power of my finders to show stars, has generally been about four times that of the eye; then, as they would show a star at the distance of the 48th order, a celestial object, situated at this distance, would require to be brought to one quarter of that distance to become visible to the eye.

The 2d cluster of the connoissance, for instance, was seen in a finder with the above mentioned power, and its profundity having been ascertained to be 243, we may conclude that it would be visible to the eye, if it were only of the 60.75th order; this being admitted, it will follow that the 20 feet telescope would still show this cluster of stars as an ambiguous object, if it were removed to the 4561st order; and with a space-penetrating power of 191.69, the 40 feet telescope, by which it was also observed, would have shown this cluster under the semblance of a star that might be distinguished from others by having rather a larger diameter, if it had been at the distance of the 11645th order.

In the foregoing instances, I have assigned the extent of the power to reach celestial objects, as it is in the same instruments whereby they were observed, but this is not a necessary condition; for when the visibility, and the particu-

lar manner of its appearance of any cluster of stars in a finder or in a small telescope of any known gaging power is ascertained; and when also by any superior instrument its profundity in space has been assigned, so that it may be reduced to the station at which it would be visible to the eye, it may then be viewed with any telescope of which the space-penetrating power is known; and if we put e for the power of the eye, f for that of the telescope which acts the part of the finder, p for the ascertained profundity of the cluster, and S for the space-penetrating power of a superior telescope, then will the extent E of this telescope to reach the same cluster, as an ambiguous object of any required appearance, be had by the formula $E = \frac{epS}{f}$.

It will not be necessary to calculate, by this formula, the order of distances at which in large telescopes some of the clusters of stars would be seen like telescopic comets; others as large stellar nebulæ, and others again as ill defined stars surrounded by nebulosity, as all these appearances must fall within the compass of the full stretch of their power; I shall therefore only add a calculation of the ambiguous visibility of one of the very distant clusters of stars.

The 75th of the connoissance is not visible to the eye, but may be seen in the finder; and the telescopic observations of it have ascertained its profundity to be of the 734th order; the station to which it should be brought, that it might be visible to the eye, is therefore of the 183.5th order. From this it follows, that with any telescope which has the space-penetrating power of the front view of my 20 feet reflector, this cluster might still be perceived if it were removed to the

distance of the 13707th order; and that the 40 feet telescope, which in this case would really act the part of a finder, would still show this cluster of stars as an ambiguous object at a profundity in space amounting to the 35175th order.

W. HERSCHEL.

Slough, near Windsor.

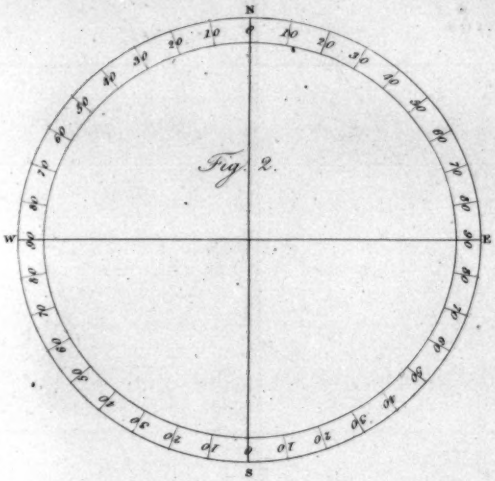


Fig. 2.

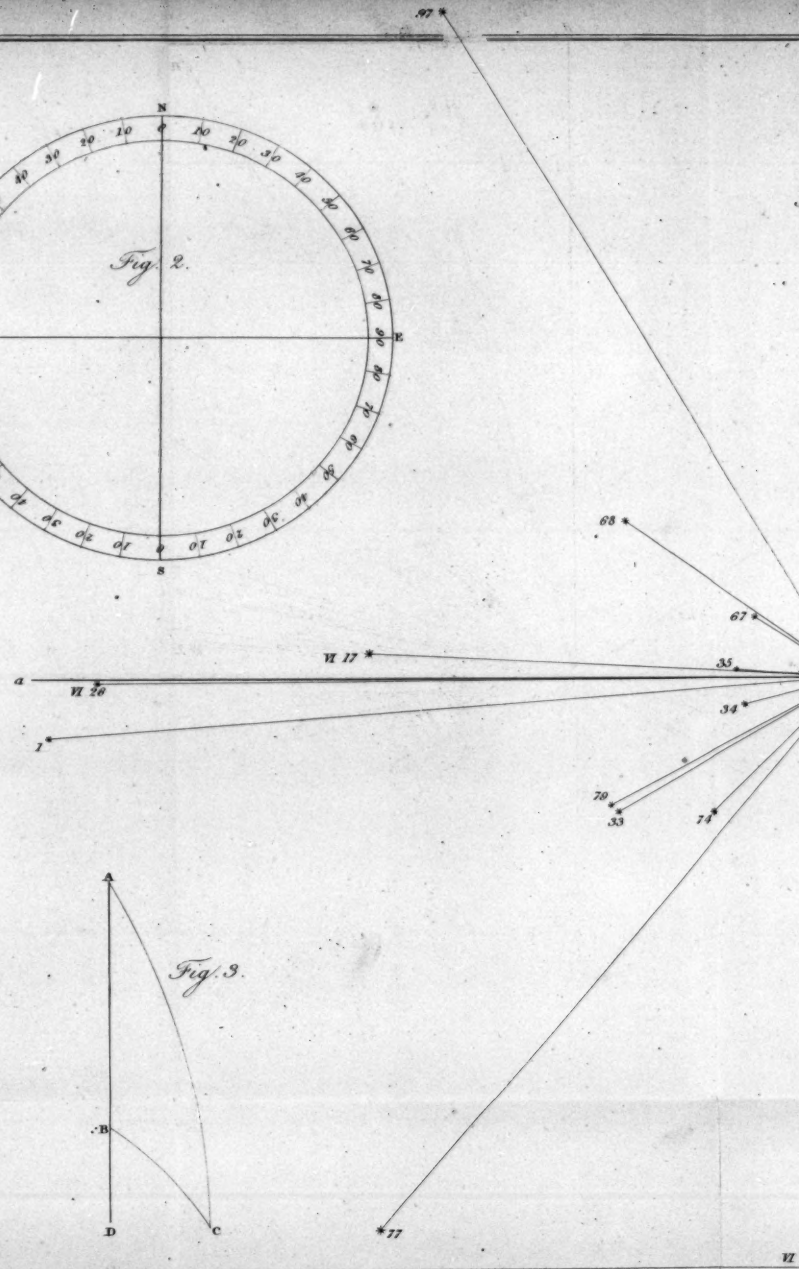


Fig. 3.

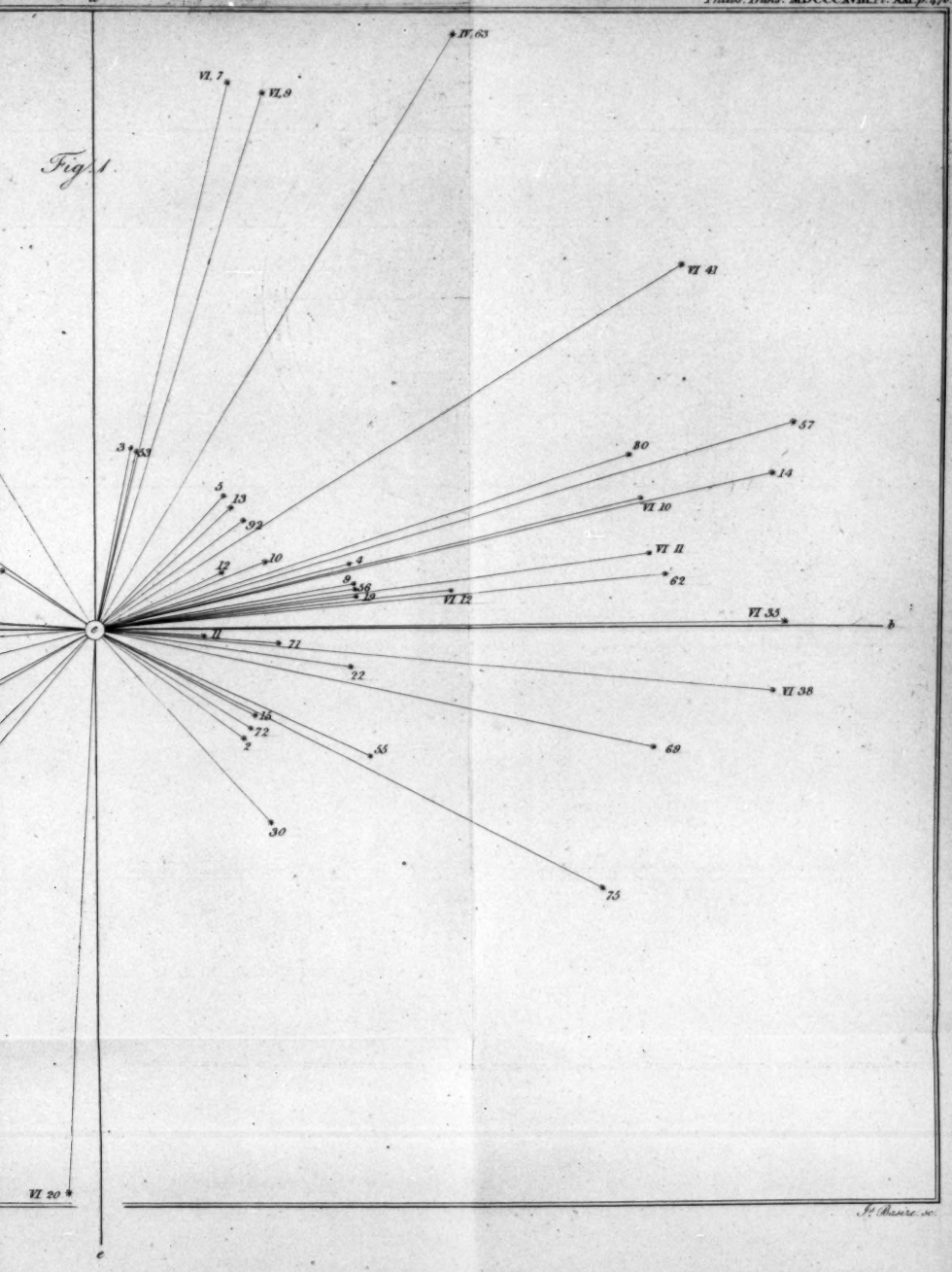


Fig. 1
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XXIV. *On the structure of the poisonous fangs of serpents.* By Thomas Smith, *Esq. F. R. S.*

Read June 4, 1818.

When the poisonous fangs of serpents are attentively examined, a slit or suture may be observed extending along the convex side, from the foramen at the base to the aperture near the point. (Plate XXII. A. B. C. D.) This is a consequence of an unusual, and hitherto, I believe, entirely unnoticed structure, resulting from the mode of formation of the tube through which the poison flows.

My attention was called to this structure, by having lately received from my friend Mr. Herbert Ryder, the assay master to the mint at Madras, the bones of the skull of a cobra de capello. I had some years since noticed the slit running along the convex side of the fang, in making a preparation of the head of the common viper of this country, in which it is distinctly seen when magnified; nevertheless, it seems to have been overlooked by all the numerous authors who have written upon the subject of the venomous fangs of the viper, and who, as far as structure is concerned, do not appear to have advanced beyond Pliny, to whom, and even anterior to whose time, the circumstance of their being tubular was well known.

All teeth being formed from a pulp, which has the shape that the tooth itself is destined to retain, it has probably been imagined that the tube of the poisonous fangs of serpents

was produced by a perforation passing through the pulp; this is not, however, the case, the tube being completely external, and formed by a deep longitudinal depression on the surface of the pulp.

In order to render this more clear, I must here observe that a slight longitudinal furrow, or depression, is to be seen on all the teeth of the cobra de capello; on those which are nearest to the poisonous fangs it is most evident, and occupies the convex side of their curvature; it however is confined entirely to the parietes of the tooth, and does not at all affect the form of its cavity.

But in the poisonous fangs, this depression is sunk deep into the substance of the tooth, and occupies a portion of the space, which in the others is allotted to the cavity which contains that part of the pulp which remains when the tooth is completely formed; and the edges of the depression being brought together along the greater part of the tooth, form the slit or suture that I have before described, but, being kept at a distance at both extremities, there results a foramen at the base and at the apex.

That this is a correct view of the mode in which the poisonous tube is formed, receives additional support from what I have observed in a species of the genus hydrus, of SCHNEIDER. In this serpent, as in many others nearly allied to it (les hydres of M. CUVIER), there are simple teeth on the same bone which supports the poisonous fangs. These teeth so much resemble the fangs, that it requires a very close investigation to distinguish between them; and this arises from the simple tooth having not only a longitudinal furrow exactly resembling the edges of the slit of the poisonous

fang, but also a very visible cavity at the base, where the foramen occurs in the others; and I have even found a fine tube in a tooth of this sort; it was however confined to the parietes, and did not affect the cavity of the tooth.

To this gradation from a slight superficial furrow to a deep depression, may be added the fact, that no traces of either are observable in the teeth of those serpents which are not armed with venomous fangs: this I found to be the case in a large species of boa.

As a consequence of the structure that I have described, if a horizontal section be made of a poisonous fang, in which the edges of the longitudinal depression are rounded, we shall have a cylindrical cavity (the poison tube) nearly surrounded by a semilunar one (the cavity which contains the pulp). This is shown in the annexed drawings of the fangs of the cobra de capello. (Pl. XXII. E. F. G. H.)

If, however, the edges of the depression should be angular (as in the rattle snake), the horizontal section shows a figure somewhat different, the poison tube being more completely surrounded by the cavity which contains the pulp. This is shown in the drawing by the section of a fang of an unknown species of serpent, which has exactly the same form as that of the rattle snake, but is twice as large. (Pl. XXII. I. K.)

In sections taken at different parts of the fang, the proportions between the poison tube and the cavity which contains the pulp will be different; the latter greatly increasing towards the base of the tooth; and near the apex the poison tube only will be seen, the fang at that part being solid. In a section also of a completely formed fang, the poison tube, at its anterior part, will be closely invested by the thickened

parietes of the cavity which contains the pulp; this cavity however is never obliterated, but exists in all the teeth of serpents, even when they have arrived at their full growth.

In the fangs, when completely formed, the edges of the slit, or suture, are frequently soldered together; when they are angular, so large a surface comes in contact, that they appear to be united by bony matter; in the cobra de capello, where they are rounded, though in very close contact, they do not cohere. In the viper, the slit seems filled up by the enamel, which being nearly transparent, a bristle in the poison tube may be seen through it, and causes an appearance as if the slit was open.

In the first case, therefore, there is no channel observable on the exterior of the tooth; the line of junction, however, of the edges of the slit is very distinctly marked: in the cobra de capello there is an external furrow from the foramen of the base to that of the apex, owing to the edges of the slit being rounded; the same is the case in those species of hydrus that I have examined.

I should observe, that the poison tube is not coated with enamel; for the membrane or capsule in which the tooth is formed, and from the inner surface of which it is well known that the enamel is deposited, does not pass between the edges of the slit into the poison tube; as, however, it passes over the slit, it will cover it with enamel, and in some cases, by that means alone, the edges become soldered together.

As some excuse for the errors which may be found in this paper, I must observe, that many of my observations have been confined to small teeth of a species of hydrus, which I was therefore obliged to dissect under the microscope.

I have to thank Sir EVERARD HOME for the great interest that he has taken in the object of my enquiry, and for the assistance which he has afforded me; on the value of which it would be needless to enlarge before the Members of this Society.

The drawings annexed to the paper will sufficiently attest my obligations to Mr. CLIFT. I owe much to him, in addition, for the zeal with which he exhibited to me every thing in the Museum of which he has the custody, that was likely to promote my views, and for information upon several points, which was required in the progress of the investigation.

DESCRIPTION OF PLATE XXII.

a, *b*, *c*, *d*, are representations of the poisonous fangs of the cobra de capello, in four stages of their growth.

A, B, C, D, are magnified representations of the same.

A, is a full grown fang firmly fixed to the bone.

B, is not quite perfect, the lower part of the foramen at the base not being yet formed.

C, in this a very small part of the foramen is formed.

D, the part of the tooth above the foramen alone appears.

E, F, G, are end views of B, C, D, showing the poison tube nearly surrounded by the cavity which contains the pulp, and the proportions between them, at three different parts of the tooth.

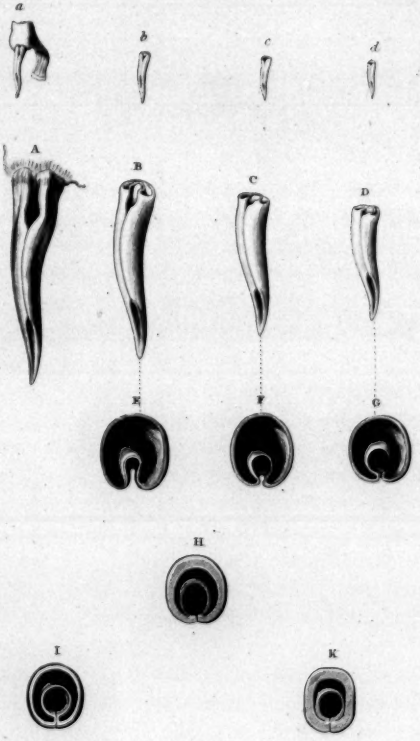
H, a section made by sawing the full grown fang A, just above the lower foramen, showing the rounded edges of the slit, which consequently leave a slight channel along the tooth.

I, K, magnified representations of sections of the fangs of

an unknown species of serpent, which have exactly the same form as those of the rattle snake.

I, is a section of a young fang taken about the middle: in this stage of growth the cavity which contains the pulp almost entirely surrounds the poison tube, and the edges of the depression which form the suture are seen to be angular, and present so large a surface to each other, that the suture is completely filled up even in this early stage of growth.

K, is a section of a full grown fang of the same species of serpent at the same part as the preceding. Here the cavity of the pulp is seen greatly contracted from the more advanced stage of growth.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XXV. *On the parallax of α Aquilæ.* By John Pond, *F.R.S. Astronomer Royal.*

Read April 16, 1818.

THE telescope erected for the investigation of the parallax of α Aquilæ, exactly resembles in its construction that which was employed for the observations of α Cygni. It has an achromatic object glass of ten feet focal length, and four inches diameter.

I had at first selected β Canis minoris, as a proper star to be compared with α Aquilæ; but I found upon trial that it could not be seen in the day time, except under such favourable circumstances as could seldom be expected. I considered this at the time as a great disappointment; but I now find that the star (*l* Pegasi) which I have substituted for it, is much better situated for the purpose. It has often been proposed to examine the parallax of a large star by comparing it with a smaller one; but to do this by meridian observations is very difficult, and in most cases impossible, from some peculiar practical difficulties which I am about to state.

For example: in the present case of α Aquilæ, the smaller star must have very nearly the same polar distance. If it follows the larger star within three hours of right ascension, it cannot be employed for this purpose, because in winter time it will pass the meridian before sun set; should the smaller star differ four hours or more in right ascension from the larger, it will in the summer pass the meridian after

sun rise, and be equally invisible. It is seldom that a star can be found within these very narrow limits; but relatively to α Aquilæ, it fortunately happens that *l* Pegasi is so situated as to be very advantageously employed.

Not being yet perfectly satisfied with respect to the stability of the instrument, I have only computed those observations in which each star has been observed on the same day. In so short an interval as three hours, I cannot conceive any sensible change of position in the telescope can possibly affect the observations. Notwithstanding this precaution, they are far inferior in exactness to those of α Cygni. I can only attribute this to the effect of accidental refraction.

From the table * which accompanies this paper, it appears that 54 observations from the 25th July to 29th December, 1817, divided into four equal series, give the following results.

Difference in declination of α Aquilæ and *l* Pegasi.

July 25, to Aug. 25, - -	1′49″,17
Aug. 25, to Sept. 8, - - -	1 49 ,20
Sept. 8, to Nov. 1, - - -	1 49 ,61
Nov. 1, to Dec. 29, - - -	1 48 ,57

If the first half be compared with the second half, the result will be,

July 25, to Sept. 8, - - -	1′49″,18
Sept. 8, to Dec. 29, - -	1 49 ,06

I can discern no appearance of parallax in the above observations; and indeed I have long considered it as a hopeless task to establish its existence by observations on a star so far from the zenith.

* Vide page 480.

After so many fruitless attempts to establish the existence of sensible parallax, I was much disposed to abandon all farther prosecution of this subject, when my anxiety was again renewed by the paper lately communicated to the Society by Dr. BRINKLEY. The arguments and observations which it contains, are such as no doubt require very attentive consideration; but I think some of Dr. BRINKLEY's doubts have arisen from my not having myself been sufficiently explicit as to the details of my own observations, and the precautions I have used. However this may be, it seemed to me more than ever desirable to institute some new process of investigation, to which none of Dr. BRINKLEY's objections could possibly apply; and it has occurred to me, that perhaps the observations made with the new transit instrument might be sufficiently exact for this purpose, though taken under very unfavourable circumstances. This was a question to be easily determined by inspection, and I have the satisfaction to state, that I find the observations of α Aquilæ, already made, quite sufficient to establish this important point; namely, that the parallax of this star is either an insensible quantity, or is so extremely small, that it cannot possibly have had any share in producing the discordances observed by Dr. BRINKLEY.*

* These observations, as likewise some additional ones on other bright stars, continued to the month of September, will form the subject of another paper.

1817.	Observed Difference.	Difference reduced to space.	Do. reduced to the beginning of 1817.		
	Rev. Parts.	′ ″	′ ″		
July 25	6.62	1.53.65	1 49.93		
26	6.56	1.52.62	48.21		
27	6.63,5	1.53.90	49.61		
28	6.56,5	1.52.63	48.55		
30	6.53	1.52.11	48.11		
Aug. 1	6.64	1.54.00	49.40		
3	6.62	1.53.65	49.17	14 = 1.49,174	
5	6.63,5	1.53.90	49.39		
6	6.68	1.54.68	49.94		
9	6.65	1.54.16	49.44		
15	6.69	1.54.85	48.60		
17	6.68	1.55.05	49.58		
22	6.73	1.55.54	49.76		
25	6.65	1.54.16	48.75		1.49,188
31	6.78	1.56.40	49.65		
Sep. 3	6.61	1.53.48	47.28		
4	6.77	1.56.32	49.84		
5	6.66	1.54.30	47.35		
6	6.78	1.56.40	49.95		
7	6.63	1.53.82	47.71		
8	6.76	1.56.06	49.64	13 = 1.49,203	
10	6.80,5	1.56.82	50.66		
21	6.86	1.57.77	50.60		
24	6.74	1.55.71	48.41		
26	6.75	1.55.88	48.71		
27	6.80	1.56.74	50.45		
28	6.82	1.57.08	49.40		
Oct. 1	6.81,5	1.56.99	50.15		
2	6.81,0	1.56.90	50.41		
3	6.84	1.57.42	49.74		
4	6.84	1.57.42	49.68		
faint 5	6.92	1.58.80	50.86		
6	6.86,5	1.57.85	49.96		
11	6.82	1.57.08	48.68		
17	6.88	1.58.11	49.90	14 = 1.49,613	
22	6.92	1.58.80	50.35		
28	6.90	1.58.46	49.70		
29	6.88	1.58.11	48.82		
30	6.84,5	1.57.50	48.30		
31	6.92,5	1.58.89	49.25		
Nov. 1	6.88	1.58.11	48.78		
5	6.87,5	1.58.03	48.44		1.49,090
11	6.93	1.58.97	49.08		
13	6.92	1.58.80	48.70		
15	6.91	1.58.62	48.08		
19	6.99,5	2. 0.10	49.37		
Dec. 7	7. 0	2. 0.18	48.02		
9	7. 2	2. 0.52	47.91	13 = 1.48,568	
11	7. 4,5	2. 0.95	48.84		
17	7.12	2. 1.70	49.19		
18	7. 2	2. 0.52	47.96		
26	7.11	2. 1.53	48.38		
27	7. 7	2. 0.85	47.88		
29	7.12	2. 1.70	48.93		

* The effect of parallax in the above observations should produce an apparent increase in the relative distance of the two stars. The extremely small difference, is in a contrary direction. No observation has been omitted in this result.

XXVI. *On the parallax of the fixed stars in right ascension.* By John Pond, *F. R. S. Astronomer Royal.*

Read May 28, 1818.

In the Connoissance des Temps for the year 1808, M. Delambre, in a memoir on the parallax of the fixed stars, in which he relates the supposed discovery of a very considerable parallax, by two celebrated Italian astronomers,* proposes that this investigation should be attempted in right ascension.

I have often read this memoir with great attention, but have been prevented by accidental circumstances from carrying this suggestion into execution. The investigation has since that time become infinitely more difficult, from the narrow limits into which parallax has been restricted by more recent observations.

When the subject first engaged my attention, I was constantly in expectation of the new instrument, and was unwilling to engage in a series of observations which would so soon be interrupted; when this was erected, I was so much engaged in the other methods of investigation, that it was not possible to devote myself properly to this enquiry; it was only lately that it occurred to me to examine the observations as they stood on the transit book for other purposes; and I find that, notwithstanding they have not been regularly made at the two opposite seasons most favourable for detecting a parallax, yet a sufficient number of observations may be collected to establish the same conclusion as that which I have given in my former paper. The stars, of which I have a sufficient number of observations for this investiga-

* Piazzi and Calandrelli.

tion, are α Aquilæ, Arcturus, Capella, and α Lyræ. Sirius is comparatively but seldom observed, from its vicinity to the horizon. As the observations of all the above stars give the same result, it will be sufficient to select one as an example. I have chosen α Aquilæ. The annexed table contains 120 observations of this star; and the following remarks will, I trust, be thought sufficiently conclusive to establish the point in question.

If a series of 12 results (obtained by 120 observations) be divided into two parts, first, according to the law of parallax, and next alternately, or in a manner perfectly accidental, then it so happens, that a greater difference is found in the latter or accidental mode of division, than in the former. From this it is evident, that the determination of the quantity of parallax is out of the power of the instrument to determine with this number of observations. The next question is, What are the limits within which parallax is restricted?

In examining several series of observations, I find that the result of 60 observations, reduced to the equator, and taken accidentally, never differs from the more correct mean derived from 120 by more than 0″,01 of time, when reduced to the equator.

If of 12 results taken as above, the 6 least be classed together, and compared with the six greatest, the error of either class will not exceed the double of this quantity, or 0″,02. It is therefore very highly improbable indeed, that an error of this magnitude should exist in a result deduced from 60 observations.

Since, therefore, the results arranged according to the law of parallax should differ by rather more than half the double parallax, I infer, that it is most highly improbable that the longer

axis of the ellipse described by parallax should, in the brighest stars, amount to 0″,5 of space, and not probable that it should amount to half this quantity, or to 0″,25. And when we consider that the minor axis of this ellipse is only measured in declination (and in α Aquilæ this is only equal to half the major axis); and that, moreover, the star is only deranged from its mean place, the half of this minor axis, I think it will not be very unsafe to conclude, that every attempt to discover the existence of parallax by a measure in declination, must end in disappointment.

These observations, continued for many years with the transit instrument, must in the end either detect the existence of parallax, or still more correctly define its limits. But these appear to me even now so small, that I am not disposed to institute any farther observations with a view to this particular subject, but shall leave it to be determined by the regular course of observation.*

* I take this opportunity of stating that the observations of α Cygni, continued in the manner described in a former paper, confirm, in the most decided manner, the total absence of any observable parallax. They are as follows:

	α Cygni.		β Aurigæ.		Difference of α and β Cygni.
	N°. of Obs.		N°. of Obs.	α Cygni & β Aurigæ	
				Sum	
Winter, 1817	25	8″,173	28	9″,818 = 17″,984	5″,226
Spring, —	26	7,920	29	10,044 = 17,964	
Summer, —	32	3,340	22	14,825 = 18,165	5,287
Autumn, —	25	4,075	17	14,067 = 18,142	
Winter, 1818	52	5,645	47	12,529 = 18,174	5,432

These observations seem to me to prove beyond a doubt that the parallax of α Cygni cannot much exceed one-tenth of a second of a degree. Vide Phil. Trans. 1817.

Observations of α Aquilæ reduced to Jan. 1, 1817.

1816.		Ⱥ Reduced.	1816.		Ⱥ. Reduced.	1817.		Ⱥ Reduced.
July	24	19h 41′ 48″.07	Nov.	3	19h 41′ 48″.10	Sept.	30	19h 41′ 51″.06
	25	48.09		8	48.03	Oct.	1	51.06
	28	48.29		9	48.14		2	50.90
	30	47.84		13	48.29		3	51.11
Aug.	3	48.17		15	48.21		6	51.16
	4	48.06		17	48.20		7	51.10
	8	48.03		18	48.14		8	51.26
	15	48.09		22	47.98		11	51.13
	18	48.06		24	48.18		17	51.06
	21	48.21		29	48.09	Nov.	11	50.85
	22	48.00		30	48.04		13	51.07
	25	48.09	Dec.	7	48.21		20	51.00
	26	47.86		8	48.14		22	51.02
	27	48.14	1817.	20	48.09		28	51.09
	28	48.18	Jan.	7	47.91		29	51.02
Sept.	2	48.20		8	48.14	Dec.	9	50.96
	3	48.05	Feb.	6	48.11		10	51.07
	4	48.09	Mar.	4	48.35		11	51.12
	5	48.23		6	48.23		15	51.10
	10	48.06		9	48.23		17	50.88
	11	48.13		10	50.94		22	51.02
	12	48.24		21	51.09		24	51.09
	14	48.14		22	51.09		26	50.95
	15	48.20	May	30	51.18		27	51.00
	16	47.92	July	28	51.10	1816.	29	51.07
	22	48.07	Aug.	5	51.02	Jan.	16	51.12
	26	48.07		6	51.12		17	50.80
	27	48.26		12	51.02		20	51.07
	30	48.23		15	51.11		24	50.21
Oct.	1	48.07		21	51.11		30	51.07
	8	48.15		22	50.97	Feb.	13	53.82
	16	48.05		29	50.98		14	54.08
	22	48.17	Sept.	6	51.06		19	54.17
	23	48.18		7	51.02		23	53.93
	26	48.13		8	51.01		28	53.96
	27	48.00		10	51.04	Mar.	1	53.91
	28	48.12		19	51.00		2	53.94
	29	48.01		21	51.15		3	54.00
	31	48.09		27	51.11		5	54.16
Nov.	1	48.13		29	51.03		7	54.00

Ann. var. 2.93 = 51.020 (Aug. 3 – Aug. 21)

48.136 + 2.93 = 51.066 (Nov. 18 – Nov. 29)

48.135 + 2.93 = 51.065 (Jan. 7 – Jan. 8)

48.153 + 2.93 = 51.083 (Aug. 15 – Aug. 18)

51.078 (May 30 – July 28)

51.040 (Jan. 16 – Jan. 30)

51.068 (Nov. 11 – Nov. 17)

51.033 (Dec. 9 – Dec. 29)

48.090 + 2.93 = 51.020 (Sept. 2 – Sept. 5)

48.103 + 2.93 = 51.033 (Sept. 16 – Sept. 22)

51.037 (Sept. 6 – Sept. 8)

53.997 + 2.93 = 51.067 (Feb. 23 – Feb. 28)

Result of 120 *Observations.*

1816. July 24 to Aug. 21	-	51″.020
Aug. 22 to Sept. 10	-	.020
Sept. 11 to Oct. 1	- -	.063
Oct. 8 to Nov. 1	-	.033
Nov. 3 to Nov. 29	-	.066
Nov. 30 to Mar. 9 (1817)		.065
1817. Mar. 10 to Aug. 21	-	.078
Aug. 22 to Sept. 29	-	.037
Sept. 30 to Nov. 11	-	.068
Nov. 13 to Dec. 17	-	.033
Dec. 22 to Jan. 30 (1818)		.040
1818. Feb. 13 to Mar. 7	-	.067
Mean	-	51 .049

Results according to the law of parallax.

Neutral.	Minimum.
51″.020	51″.020
.065	.063
.078	.033
.033	.066
.040	.037
.067	.068
Mean 51 .050	51 .048
51 .049	51 .049
Error = .001	Error .001

Taken alternately or accidentally.

51″.020	51″.020
.063	.033
.066	.065
.078	.037
.068	.033
.040	.067
Mean 51 .056	51 .043
51 .049	51 .049
Error 0 .007	Error = 006

Six least.	Six greatest.
51″.020	51″.063
.020	.066
.033	.065
.033	.078
.037	.068
.040	.067
Mean 51 .030	51 .068
51 .049	51 .049
Error = .019	Error = .019

XXVII. *An abstract of the results deduced from the measurement of an arc on the meridian, extending from latitude 8° 9′ 38″,4, to latitude 18° 3′ 23″,6, N. being an amplitude of 9° 53′ 45″,2. By Lieut. Colonel* William Lambton, *F. R. S. 33d Regiment of foot.*

Read May 21, 1818.

In the 12th vol. of the Asiatick Researches, there are detailed accounts of two complete sections of an arc on the meridian, measured by me at different times, in prosecuting the trigonometrical survey of the Peninsula of India. The first is comprehended between the parallels of Punnæ, a station near Cape Comorin, in latitude 8° 9′ 38″,30, and Patchipolliam in Coimbetoor, in latitude 10° 59′ 48″,93; being an amplitude of 2° 50′ 10″,54. The second is comprehended between the parallels of Patchipolliam and Namthabad, a station near Gooty, in the ceded districts; and lying in latitude 15° 6′ 0″,21, gives an amplitude of 4° 6′ 11″,28. Since these measurements were made, I have had the good fortune to get another section, extending from Namthabad to Daumergidda, in the Nizam's dominions, which being in latitude 18° 3′ 23″,6, gives an increase of 2° 57′ 23″,32; making in the whole an arc of 9° 53′ 45″,14 in amplitude; the longest *single* arc that has ever been measured on the surface of this globe. The detailed accounts of this last arc, with various conclusions concerning the three sections, have been presented to the Asiatick Society by the Marquess of Hastings, the present Governor General, and will be published in the 13th volume

of the Asiatick Researches. But as it may be three or four years before that volume makes its appearance, I have been induced to draw out this abstract of the results, thinking that the conclusions herein contained may be interesting to the Royal Society, and to the astronomers of Europe. As a reference may be made to the volumes above mentioned, I have simply given the lengths of the sides of the triangles from which the arcs are deduced, together with the lengths of the terrestrial and celestial arcs, without including either the tables of triangles, or the particulars of the astronomical observations, farther than the names of the stars, and the mean lengths of the celestial arcs, and, consequently, the mean degrees deduced therefrom.

The first of these sections gives the degree due to latitude 9° 34′ 44″, the middle point of that arc, equal 60472,83 fathoms. The second section, whose middle point is in latitude 13° 2′ 55″, gives the mean degree equal 60487,56 fathoms. The last section gives the degree equal 60512,78 fathoms, due to the latitude of 16° 34′ 42″, the middle point of that section.

In my second paper, in the 19th vol. of the Asiatick Researches, it appeared that the degree due to latitude 11° 37′ 49″, the middle point between Punnæ and Namthabad, was 60480,3 fathoms. Since that paper was sent, there has been a small correction applied to the base near Gooty, after comparing the chains with the brass standard scale. This correction has somewhat increased the meridional distance between that base and Yerracondah south; and, consequently, the whole terrestrial arc between Namthabad and Punnæ is also increased, which now gives the degree due to latitude

11° 37′ 49″, equal 60481,55 fathoms. However, as there are now three distinct sections, whose respective middle points lie in 9° 34′ 44″; 13° 2′ 55″; and 16° 34′ 42″; I have thought it best to take the degrees due to these latitudes, as deduced from actual observation, using each, *first* with the French measure, *then* with the English, and *lastly* with the Swedish measure; and thence obtaining a general mean for the compression at the poles. The *first mean* of these three degrees used with the French degree, gives the compression $\frac{1}{309,15}$. The *second mean* of the same three degrees used with the English degree, gives $\frac{1}{313,54}$. And the *third* mean of these three degrees used with the Swedish degree, gives $\frac{1}{307,19}$ for the compression; so that the mean of these three means will give the compression at the poles $\frac{1}{309,96}$, or $\frac{1}{310}$ nearly of the polar axes; and this has been finally adopted for computing the general tables of degrees from the equator to the pole.

It will be seen by inspecting the plan of the triangles, (Pl. XXIII.) that all the sides from which the arc has been deduced lie so near the meridian, that no correction has been required; a circumstance that has saved much trouble. The sides being so nearly north and south, that the base reduced from each side as an hypothenuse, may be considered as a chord of an arc parallel, and so nearly contiguous to the meridian, that it may, as to sense, be taken as the chord to the same arc on the meridian; and these chords being in general short, they will be the same as the arcs, very nearly. I have therefore not been at the trouble of applying any correction; for if the whole arc between Punnæ and Daumergidda (upwards of 680 miles) were divided into small arcs of 30

miles each, the whole difference between these arcs and their chords would not be more than a fathom and a half.

The number of base lines in this extensive arc, are five; all measured with the chain extended in coffers; with elevating screws, &c; and every part of the operation has been performed with the greatest posssible attention. The one near Bangalore may be considered as the first; and its height above the sea was obtained by a series of triangles connecting it with another base near St. Thomas's Mount, whose height above the low water mark was determined by observations made at the sea beach, and at the race stand near the north end of the base (Asiat. Res. vol.viii.). The base lines to the southward are, the one in Coimbetoor, and the other near Tinnivelly, whose heights above the sea were determined from the Bangalore base. Those to the northward are, the base near Gooty, and the one near Daumergidda. The account of this last measurement, and of the curious experiments for comparing the steel chains with the brass standard scale, will appear in the 13th volume of the Asiatick Researches. The particulars of the other measurements may be seen in the 10th and 12th volumes of the same work.

The great station of observation at Doddaguntah, is near the first base line; and it was at that station where the position of the meridian was fixed for extending it to the north and south. The latitude of that station was also determined by observing the zenith distances of a number of stars from the Greenwich catalogue for 1802. That latitude, however, was afterwards set aside from a supposed disturbance of the plummet. The latitude was afterwards fixed from observations made at Punnæ, the southernmost station of

observation, by setting off degrees corresponding to different latitudes, after they were finally determined.

As Doddaguntah station has been left out in the divisions of the grand arc, its latitude was only useful in fixing that of Savendroog, one of the great meridian stations for crossing the Peninsula (Asiatick Researches, vol. i.). The other remaining stations of observation are, Patchipolliam, or Coimbetoor; Namthabad, near Gooty; and Daumergidda, in the Nizam's dominions. We shall now see how these stations are connected by the triangles; Doddaguntah being the referring station in counting the northings, southings, &c.; and to whose meridian the whole terrestrial arc is reduced.

Without regarding the order of time, we will set off from Doddaguntah to Patchipolliam, and thence to Punnæ. After which we will return to Doddaguntah, and proceed northerly to Namthabad, and from Namthabad to Daumergidda.

TABLE I. *Lengths of the terrestrial arc comprehended between the parallels of Doddagun tah station, and the station near Patchipolliam.*

Stations at	Stations to	Bearings	Distances	Distances on the		Distance from Doddaguntah	
				Perpendicular	Meridian	Perpendicular	Meridian
Doddaguntah	Deorbetta	0° 13′ 08″,43 SE	135931,3	519,6 E	135930,3 S	519,6 E	135930,3 S
Deorbetta	Ponnasmalli	2 11 54,36 SE	174071,7	6677,6 E	173947,6 S	7197,2 E	309877,9 S
Ponnasmalli	Woorachmalli	3 49 54,39 SE	243502,4	16272,6 E	242958,2 S	23469,8 E	552836,1 S
Woorachmalli	Patchipolliam	7 53 51,52 SW	276169,4	24206,4 W	174498,5 S	736,6 W	7273346,6 S

TABLE II. *Length of the terrestrial arc comprehended between the parallels of Doddaguntah station and the station at Namthabad.*

Stations at	Stations to	Bearings	Distances	Distances on the		Distance from Doddaguntah	
				Perpendicular	Meridian	Perpendicular	Meridian
Deorbetta	Allasoor hill	0° 43′ 54″,55 NW	194662,8	2486,3 W	194646,9 N	1966,7 W	58716,6 N
Allasoor hill	Kulcottah hill	4 5 43,25 NW	94211,8	6728,3 W	93971,2 N	8695,0 W	152687,8 N
Kalcottah hill	Yerracondah	5 43 49,55 NE	180883,8	18060,9 E	179979,9 N	9305,9 E	332667,7 N
Yerracondah	Ooracondah	7 04 21,49 NW	126785,7	15610,8 W	125821,0 N	6244,9 W	458488,7 N
Ooracondah	Davurcondah	5 32 52,09 NE	150506,1	14550,4 E	149801,1 N	8305,5 E	628289,8 N
Davurcondah	Gooty Droog	0 16 40,56 NE	158946,2	771,1 E	158944,3 N	9076,6 E	767234,1 N
Gooty Droog	Namthabad	70 43 30,60 SW	16472,2	15548,9 W	5437,5S	6472,3 W	761796,6 N

TABLE III. *Length of the terrestrial arc between the parallels of Patchipolliam and the station near Punnæ.*

Stations at	Stations to	Bearings	Distances	Distances on the		Distance from Doddaguntah	
				Perpendicular	Meridian	Perpendicular	Meridian
Patchipolliam	Parteemalli	7° 56′ 29″,74 SW	120553,6	16656,1 W	119397,4 S	16656,1 W	119397,4 S
Parteemalli	Peermaulmalli	1 54 37 ,23 SW	133318,9	4444,2 W	133244,8 S	21100,3 W	252642,2 S
Peermaulmalli	Sudragherry	11 14 52 ,96 SE	207080,1	40392,4 E	203102,5 S	19212,1 E	455744,7 S
Sudragherry	Vulunkota	3 13 50 ,25 SW	330403,5	19127,1 W	338864,1 S	165,0 E	794608,8 S
Vulunkota	Kunnamopolha	0 10 35 ,28 SW	108363,9	333,8 W	108363,3 S	168,8 W	902972,1 S
Kunnamopolha	Punnæ station	0 27 21 ,16 SE	126132,4	1003,6 E	126128,4 S	834,8 E	1029100,5 S

TABLE IV. *Length of the terrestrial arc comprehended between the parallels of Doddaguntah and Daumergidda.*

Stations at	Stations to	Bearings	Distances	Distances on the		Distance from Doddaguntah	
				Perpendicular	Meridian	Perpendicular	Meridian
Gooty Droog	Koelcondah	10° 59′ 34″,9 NW	76750,9	15635,6 W	75342,5 N	5559,0 W	842576,6 N
Koelcondah	Poolycondah	4 06 33 ,9 NW	54084,1	3875,7 W	53945,0 N	9434,7 W	896521,6 N
Poolycondah	Kerrabellagal	13 21 56 ,9 NE	127920,1	29570,9 E	124455,3 N	20136,2 E	1020976,9 N
Kerrabellagal	Daroor hill	3 04 35 ,9 NW	150701,3	8088,4 W	150484,1 N	12047,8 E	1171461,0 N
Daroor hill	Inpahgut	0 45 15 ,7 NW	175159,1	2306,1 W	175144,0 N	9741,7 E	1346605,0 N
Inpahgut	Kotakodangul	1 42 38 ,8 NW	153803,3	4593,5 W	153794,7 N	5148,2 E	1500399,7 N
Kotakodangul	Shelapilly hill	2 20 25 ,7 NE	231767,9	9464,9 E	231574,5 N	14613,1 E	1731974,2 N
Shelapilly hill	Daumergidda	0 01 33 ,0 NE	103250,6	46,6 E	103250,6 N	14659,7 E	1835224,8 N

From the foregoing tables we collect the following particulars.

	Terrestrial arcs.	
	feet	fathoms
By Table I. the terrestrial arc between Doddaguntah and Patchipolliam is - - -	727334,6	
By Table II. The arc between Doddaguntah and Namthabad is - - - - - -	761796,6	
Their sum will be the arc between Patchipolliam and Namthabad - - - =	1489131,2	248188,53
By Table III. the arc between Patchipolliam and Punnæ station is - - - - - -	1029100,5	171516,75
Their sum is the terrestrial arc between Punnæ and Namthabad - - - =	2518231,7	
By Table IV. the arc between Doddaguntah and Daumergidda is - - -	1835224,8	
From which subtract the arc between Doddaguntah and Namthabad - - - -	761796,6	
The difference will be the arc between Namthabad and Daumergidda - - - =	1073428,2	178904,70
And the sum of the whole of these three sections will be the terrestrial arc between Punnæ and Daumergidda - - - - - -		598609,98

Amplitudes of the celestial arc between the parallels of Punnæ and Patchipolliam.

Stars.	Zenith distances at		Amplitudes.
	Patchipolliam.	Punnæ.	
ϑ Hydræ	4° 37′ 12″,65 S	1° 47′ 01″,37 S	2° 50′ 11″,28
ι Hydræ	3 52 08 ,97 S	1 01 59 ,31 S	9 ,66
α Cancri	1 36 32 ,64 N	4 26 42 ,91 N	10 ,27
ο Leonis	0 13 18 ,16 S	2 36 52 ,07 N	10 ,23
Regulus	1 55 12 ,99 N	4 45 24 ,06 N	11 ,07
θ Leonis	5 29 54 ,26 N	8 20 03 ,44 N	9 ,18
β Leonis	4 39 59 ,40 N	7 30 11 ,59 N	12 ,19
ι Virginis	1 00 55 ,20 N	3 51 05 ,95 N	10 ,75
ϑ Serpentis	0 12 14 ,15 N	3 02 25 ,36 N	11 ,21
α Serpentis	3 56 48 ,46 S	1 06 38 ,10 S	10 ,36
α Herculis	3 37 38 ,58 N	6 27 48 ,35 N	9 ,77
α Ophiuchi	1 43 00 ,69 N	4 33 11 ,86 N	11 ,17
ι Aquilæ	2 35 16 ,44 N	5 25 29 ,25 N	12 ,81
γ Aquilæ	0 50 50 ,74 S	1 59 19 ,11 N	10 ,51
Atair	2 37 54 ,13 S	0 12 14 ,69 N	8 ,82
β Aquilæ	5 03 55 ,68 S	2 13 48 ,40 S	7 ,28
β Delphini	2 55 48 ,68 N	5 45 58 ,28 N	12 ,60
		Mean	2 50 10 ,55

Amplitude of the celestial arc between the parallels of Patchipolliam and Namthabad.

Stars.	Zenith distances at Patchipolliam.	Zenith distances at Namthabad.	Amplitudes.
ε Leonis	0°13′ 18″,16 S	4° 19′ 29″,91 S	4° 06′ 11″,75
Regulus	1 55 12 ,99 N	2 10 59 ,16 S	12 ,15
δ Leonis	5 29 54 ,26 N	1 23 42 ,08 N	12 ,18
β Leonis	4 39 59 ,40 N	0 33 49 ,17 N	10 ,23
ι Virginis	1 00 55 ,20 N	3 58 56 ,58 S	10 ,07
δ Serpentis	0 12 14 ,15 N	3 53 56 ,58 S	10 ,73
α Herculis	3 37 38 ,58 N	0 28 34 ,09 S	12 ,67
α Ophiuchi	1 43 00 ,69 N	2 23 10 ,99 S	11 ,68
ι Aquilæ	2 35 16 ,44 N	1 30 53 ,02 S	10 ,06
γ Aquilæ	0 50 50 ,74 S	4 57 02 ,59 S	11 ,80
Atair	2 37 54 ,13 S	6 44 07 ,19 S	13 ,06
β Delphini	2 55 45 ,68 N	1 10 23 ,40 S	9 ,08
		Mean	4 06 11 ,28

Amplitude of the celestial arc between the parallels of Namthabad and Daumergidda.

Stars.	Zenith distances at Namthabad.	Zenith distances at Daumergidda.	Amplitudes.
ε Leonis	4° 19′ 30″,079 S	7°16′ 53″,233 S	2°57′ 23″,254
Regulus	2 10 59 ,463 S	5 08 21 ,582 S	22 ,099
γ Leonis	5 43 28 ,704 S	2 46 03 ,056 N	25 ,648
ζ Leonis	1 23 42 ,038 N	1 33 40 ,925 S	22 ,963
β Leonis	0 33 49 ,174 N	2 23 34 ,324 S	23 ,498
ι Virginis	3 05 14 ,750 S	6 02 36 ,570 S	21 ,820
κ. Bootis	4 16 54 ,460 N	1 19 29 ,335 N	25 ,125
Arcturus	5 06 16 ,777 N	2 08 51 ,502 N	25 ,272
ι Bootis	0 31 35 ,868 S	3 28 55 ,050 S	19 ,182
δ Serpentis	3 53 56 ,290 S	6 51 19 ,536 S	23 ,246
β Serpentis	0 56 32 ,646 N	2 00 50 ,287 S	22 ,933
γ Serpentis	1 12 23 ,718 N	1 45 00 ,865 S	24 ,583
γ Herculis	4 31 19 ,103 N	1 33 55 ,469 N	23 ,634
		Mean	2 57 23 ,320

Amplitude of the whole celestial arc between the parallels of Punnæ and Daumergidda.

Stars.	Zenith distances at Punnæ.	Zenith distances at Daumergidda.	Amplitudes.
ε Leonis	2°36′ 51″,926 N	7°16′ 53″,233 S	9°53′ 45″,159
Regulus	4 45 23 ,979 N	5 08 21 ,582 S	45 ,561
θ Leonis	8 20 03 ,213 N	1 33 40 ,925 S	44 ,138
β Leonis	7 30 11 ,608 N	2 23 34 ,324 S	45 ,932
ι Virginis	3 51 06 ,083 N	6 02 36 ,570 S	42 ,653
δ Serpentis	3 02 25 ,643 N	6 51 19 ,536 S	45 ,179
γ Serpentis	8 08 47 ,269 N	1 45 00 ,865 S	48 ,134
		Mean	9 53 45 ,251

Latitude of Punnæ station deduced from the foregoing zenith distances of eight principal stars, whose declinations and annual variations are given in the Greenwich observations for 1802.

Stars.	For the beginning of 1805. Mean declination.	For the beginning of 1805. Correct. zen. dist.	Latitudes.
Regulus	12°54′ 58″,93 N	4°45′ 24″,09 N	8°09′ 34″ ,84 N
β Leonis	15 39 45 ,28	7 30 11 ,59 N	33 ,70
α Serpentis	7 03 00 ,30	1 06 38 ,10 S	38 ,40
α Herculis	14 37 30 ,96	6 27 48 ,35 N	42 ,61
α Ophiuchi	12 42 50 ,91	4 33 11 ,86 N	39 ,05
γ Aquilæ	10 08 58 ,34	1 59 19 ,77 N	38 ,57
Atair	8 21 53 ,53	0 12 14 ,69 N	38 ,84
β Aquilæ	5 55 52 ,71	2 13 48 ,40 S	41 ,11
		Mean	8 09 38 ,39

Latitude of Punnæ station - -	8° 09′ 38″,39
Celestial arc between Punnæ and Patchipolliam	2 50 10 ,54
Their sum is the latitude of Patchipolliam	11 59 48 ,93

Celestial arc between Patchipolliam and Namthabad - - - -	4° 06′ 11″,28
Which added to the last gives the latitude of Namthabad - - -	15 06 00 ,21
Celestial arc between Namthabad and Daumergidda - - - - - -	2 57 23 ,32
Which added gives the latitude of Daumergidda - - - -	18 03 23 ,53
The arc between Punnæ and Daumergidda by seven corresponding stars (9° 58′ 45″,25) added to the latitude of Punnæ (8° 09′ 38″, 39) gives - - - -	18 03 23 ,64
Mean of these two latitudes gives the correct latitude - - - -	18 03 23 ,58

By comparing the above three sections of the celestial arc with their respective terrestrial measures, we shall have the following conclusions.

Celestial arc between Punnæ and Patchipolliam	2° 50′ 10″,54
Latitude of the middle point (9° 34′ 48″, 6)	9 34 44
Terrestrial arc in fathoms - -	171516 ,75
Mean length of the degree due to latitude 9° 34′ 44″ in fathoms - - -	60472 ,83
Celestial arc between Patchipolliam and Namthabad - - - -	4° 06′ 11″,28
Latitude of the middle point - -	13 02 55
Terrestrial arc in fathoms - -	248188,53
Mean degree due to latitude 13° 2′ 55″ -	60487,56
Celestial arc between Namthabad and Daumergidda - - - -	2° 57′ 23″,32

Terrestrial arc in fathoms	- -	178904,70
Latitude of the middle point	16° 34′ 42″	
Mean degree in fathoms due to latitude	16° 34′ 42″	60512,78

So that by the above comparisons it appears that the

degree due to latitude	- 9° 34′ 44″ is -	60472,83
the degree due to latitude	12° 2′ 55″ is -	60487,56
the degree due to latitude	16° 34′ 42″ is -	60512,78

It now remains to compare each of these mean degrees, *first* with the French measurement; *then* with the English; and *lastly* with the Swedish; and by proceeding on the elliptic theory, deduce from these data three mean ellipticities, and from these three *a general mean*, which must give nearly the true compression at the poles.

Previous to this determination, it will be necessary to investigate the requisite formulæ for obtaining the compression by a comparison of measured degrees in distant latitudes; and first, by the measured degrees on the meridian.

Let m' and m be the measured degrees, in latitudes $'l$ and l; and let a represent the equatorial diameter, and b the polar axis: that is, supposing the earth to be an ellipsoid, let a and b represent the transverse and conjugate axes of an elliptic meridian. Then it is known from conic sections and the nature of curvature, that $\frac{a^2 b^2}{2 (\text{Cos.}^2 {}'l . a^2 + \text{Sin.}^2 {}'l . b^2)^{\frac{3}{2}}}$ is the radius of curvature of the ellipse at $'l$; and that $\frac{a^2 b^2}{2 (\text{Cos.}^2 l . a^2 + \text{Sin.}^2 l . b^2)^{\frac{3}{2}}}$ is the radius of curvature of the same, or any other elliptic meridian, on the same ellipsoid, at the point l. And since the degrees at $'l$ and l are as the radii of curvature at these points, we have $m' : m :: \frac{a^2 b^2}{2 (\text{Cos.}^2 {}'l . a^2 + \text{Sin.}^2 {}'l . b^2)^{\frac{3}{2}}} \cdot \frac{a^2 b^2}{2 (\text{Cos.}^2 l . a^2 + \text{Sin.}^2 l . b^2)^{\frac{3}{2}}}$

$:: (\text{Cos.}^2 {}'l.\ a^2 + \text{Sin.}^2 {}'l.\ b^2)^{-\frac{1}{2}} : (\text{Cos.}^2 l.\ a^2 + \text{Sin.}^2 l.\ b^2)^{-\frac{1}{2}}$. Now to simplify this expression, if $a = 1$, e the ellipticity; and therefore $b = 1 - e$, and $b^2 = 1 - 2e$ nearly, because e^2 must be very small. Then will $m' : m :: (\text{Cos.}^2 {}'l + (1 - 2e).\ \text{Sin.}^2 {}'l)^{-\frac{1}{2}} : (\text{Cos.}^2 l + (1 - 2e).\ \text{Sin.}^2 l)^{-\frac{1}{2}}$. And if $(1 - \text{Sin.}^2 {}'l)$ and $(1 - \text{Sin.}^2 l)$ be substituted for $\text{Cos.}^2 {}'l$ and $\text{Cos.}^2 l$. the expression will be transformed into $(1 - 2e.\ \text{Sin.}^2 {}'l)^{-\frac{1}{2}}$ and $(1 - 2e.\ \text{Sin.}^2 l)^{-\frac{1}{2}}$; or $1 + 3e.\ \text{Sin.}^2 {}'l$ and $1 + 3e.\ \text{Sin.}^2 l$ nearly, by developing the series, and leaving out all the quantities involving e^2 or its higher powers. Hence

$$m' : m :: 1 + 3e.\ \text{Sin.}^2 {}'l : 1 + 3e.\ \text{Sin.}^2 l \quad . \quad (1)$$

which reduced gives

$$e = \frac{m' - m}{3\,(m.\ \text{Sin.}^2 {}'l - m'.\ \text{Sin.}^2 l)} \quad . \quad (2)$$

and when the degrees are contiguous, or very near to each other, this expression may be rendered still more simple by making $m' = m$ in the denominator, which under these circumstances will scarcely affect the result: whence

$$e = \frac{m' - m}{3m\ (\text{Sin.}^2 {}'l - \text{Sin.}^2 l)}. \quad (3)$$

By this expression it will be easy to estimate the increments to degrees lying contiguous to each other: for if m, m', m'', &c. be contiguous degrees in latitudes l; $'l$, $''l$, &c. that is l, $l + 1°$; $l + 2°$ &c. Then we shall have $e = \frac{m'' - m}{3(\text{Sin.}^2 {}''l - \text{Sin.}^2 l)}$; which being made equal to $\frac{m' - m}{3\ (\text{Sin.}^2 {}'l - \text{Sin.}^2 l)}$ and reduced, we get $m' - m : m'' - m :: \text{Sin.}^2 {}'l - \text{Sin.}^2 l : \text{Sin.}^2 {}''l - \text{Sin.}^2 l$; and in like manner $m'' - m : m''' - m :: \text{Sin.}^2 {}''l - \text{Sin.}^2 l : \text{Sin.}^2 {}'''l - \text{Sin.}^2 l$; and $m''' - m : m'''' - m :: \text{Sin.}^2 {}''''l - \text{Sin.}^2 l : \text{Sin.}^2 {}'''''l - \text{Sin.}^2 l$. &c. from which it appears that the increments to the degrees, beginning with the lowest latitude, will always be as the increments to the

squares of the sines of the corresponding latitudes: and if m be at the equator where the Sin. l is o, then we shall have $m' - m : m'' - m :: \text{Sin.}^2 'l : \text{Sin.}^2 ''l$.

Since by equation 1, $m' : m :: 1 + 3e.\ \text{Sin.}^2 'l : 1 + 3e.\ \text{Sin.}^2 l$; then

$$m' = m\left(\frac{1 + 3e.\ \text{Sin.}^2 'l}{1 + 3e.\ \text{Sin.}^2 l}\right) \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$\text{and } m = m'\left(\frac{1 + 3e.\ \text{Sin.}^2 l}{1 + 3e.\ \text{Sin.}^2 'l}\right) \quad . \quad . \quad . \quad . \quad (5)$$

When m is at the equator, and therefore Sin. $l = o$; then

$$m' = m\,(1 + 3e.\ \text{Sin.}^2 'l) \quad . \quad . \quad . \quad (6)$$

If m' be at the pole, and therefore Sin.$^2 'l = 1$, then we have

$$m' = m\left(\frac{1 + 3e}{1 + 3e.\ \text{Sin.}^2 l}\right) \quad . \quad . \quad (7)$$

If the degrees perpendicular to the meridian be made use of, let p' and p be the measures of those degrees in latitudes $'l$ and l, then the radius of curvature of the perpendicular degree at $'l$ being as $\frac{1}{2(1 - 2e.\ \text{Sin.}^2 'l)^{\frac{1}{2}}} = \frac{1}{2}(1 - 2e.\ \text{Sin.}^2 'l)^{\frac{1}{2}} = \frac{1}{2}\,(1 + e.\ \text{Sin.}^2 'l)$ very nearly; and for the same reason the radius of curvature of the perpendicular degree at l will be as $\frac{1}{2}\,(1 + e.\ \text{Sin.}^2 l)$ very nearly; so that we get $p' : p :: 1 + e.\ \text{Sin.}^2 'l : 1 + e.\ \text{Sin.}^2 l$ . . . . . (8)

and when reduced gives $e = \frac{p' - p}{p.\ \text{Sin.}^2 'l - p'\ \text{Sin.}^2 l}$ . . . (9)

$$\text{From equation 8, } p' = p\left(\frac{1 + e.\ \text{Sin.}^2 'l}{1 + e.\ \text{Sin.}^2 l}\right) \quad . \quad . \quad . \quad (10)$$

$$\text{and } p = p'\left(\frac{1 + e.\ \text{Sin.}^2 l}{1 + e.\ \text{Sin.}^2 'l}\right) \quad . \quad . \quad . \quad (11)$$

If p be on the equator where the Sin. l vanishes, then equation 10 becomes $p' = p\,(1 + e.\ \text{Sin.}^2 'l)$ . . . (12)

If p' be at the pole, and therefore the cosine of $'l$ unity, then equation 10 becomes $p' = p\left(\frac{1 + e}{1 + e.\ \text{Sin.}^2 l}\right)$ . . . (13)

Since the degree on the meridian, and the degree perpendicular to the meridian, are equal at the pole, we shall have by equations 7, and 13, $p\left(\frac{1+e}{1+e.\ \text{Sin.}^2 l}\right) = m\left(\frac{1+3e}{1+3e.\ \text{Sin.}^2 l}\right)$, where p and m are in the same latitude l. Now $m\left(\frac{1+3e}{1+3e.\ \text{Sin.}^2 l}\right) = m\,(1+3e).(1+3e.\ \text{Sin.}^2 l)^{-1} =$ $= m\,(1+3e.\ \text{Cos.}^2 l)$ nearly; and therefore $p\left(\frac{1+e}{1+e\ \text{Sin.}^2 l}\right) =$ $= p(1+e).(1+e.\ \text{Sin.}^2 l)^{-1} = p\,(1+e.\ \text{Cos.}^2 l)$ nearly. Hence $m\,(1+3e.\ \text{Sin.}^2 l) = p\,(1+e.\ \text{Cos.}^2 l)$ which reduced gives $e = \frac{p-m}{(3m-p)\ \text{Cos.}^2 l}$ . . . . (14)

Since $m\,(1+3e.\ \text{Cos.}^2 l) = p\ (1+e.\ \text{Cos.}^2 l)$, we get

$$m : p :: 1+e\ \text{Cos.}^2 l : 1+3e\ \text{Cos.}^2 l \quad . \quad . \quad (15)$$

and when $l = 0$, and its Cos. equal 1, then $m : p :: 1+e :$

$$1+3e \quad . \quad . \quad . \quad . \quad (16)$$

If we make use of the degrees of longitude, then let d' and d represent their measures in latitudes $'l$ and l; and their respective radii of curvature at $'l$ and l will be expressed by $\frac{\text{Cos.}\ 'l}{2\,(\text{Cos.}^2 'l + (1-2e)\ \text{Sin.}^2 'l)^{\frac{1}{2}}}$ and $\frac{\text{Cos.}\ l}{2\,(\text{Cos.}^2 l + (1-2e).\ \text{Sin.}^2 l)^{\frac{1}{2}}}$, and therefore $d' : d :: \frac{\text{Cos.}\ 'l}{(\text{Cos.}^2 'l + (1-2e)\ \text{Sin.}^2 'l)^{\frac{1}{2}}} ; \frac{\text{Cos.}\ l}{(\text{Cos.}^2 l + (1-2e)\ \text{Sin.}^2 l)^{\frac{1}{2}}}$; that is $d' : d :: \frac{\text{Cos.}\ 'l}{(1-2e.\ \text{Sin.}^2 'l)^{\frac{1}{2}}} ; \frac{\text{Cos.}\ l}{(1-2e.\ \text{Sin.}^2 l)^{\frac{1}{2}}}$; that is $d' :$ $d :: \text{Cos.}\ 'l\,(1-2e.\ \text{Sin.}^2 'l)^{-\frac{1}{2}} ; \text{Cos.}\ l\,(1-2e.\ \text{Sin.}^2 l)^{-\frac{1}{2}}$; that is

$$d' : d :: \text{Cos.}\ 'l\,(1+e.\ \text{Sin.}^2 'l) : \text{Cos.}\ l\,(1+e.\ \text{Sin.}^2 l) \ . \ . \ (17)$$

and this reduced gives $e = \frac{d'\ \text{Cos.}\ l - d.\ \text{Cos.}\ 'l}{d.\ \text{Cos.}\ 'l.\ \text{Sin.}^2 l - d'.\ \text{Cos.}\ l.\ \text{Sin.}^2 'l}$ (18)

since $d' : d :: \text{Cos.}\ 'l\,(1+e.\ \text{Sin.}^2 'l) : \text{Cos.}\ l\,(1+e.\ \text{Sin.}^2 l)$; if d be at the equator where Sin. l vanishes; then

$$d : d' :: 1 : \text{Cos.}\ 'l\,(1+e.\ \text{Sin.}^2 'l) \ . \ . \ . \ . \ (19)$$

From this equation we get $d = \frac{d'}{\text{Cos.}\,'l\,(1 + e.\,\text{Sin.}^2\,'l)}$.

And from equation 9 we get $p = \frac{p'}{1 + e.\,\text{Sin.}^2\,'l}$; and since at the equator the degree of longitude, and the perpendicular degree are equal, then $\frac{d'}{\text{Cos.}\,'l\,(1 + e.\,\text{Sin.}^2\,'l)} = \frac{p'}{1 + e.\,\text{Sin.}^2\,'l}$, and this reduced we shall have $d' = p'.\,\text{Cos.}\,'l$, where d' and p' are in the same latitude. Hence $d' : p' :: \text{Cos.}\,'l : \text{rad} \ldots (20)$

I shall now proceed to determine the compression at the poles from the foregoing three sections of the arc; and comparing each, *first* with the French degree in latitude 47°30′46″, equal 60779 fathoms; *then* with the English degree in latitude 52° 2′ 20″, which is 60820 fathoms; and *lastly* with the Swedish degree in latitude 66° 20′ 12," when it is 60955 fathoms.

With respect to the French degree, as there appears much irregularity in the different sections of the arc between Dunkirk and Montjouy, I have used *that* between the Pantheon at Paris, and Eveaux, as given by De Lambre in the *Base du Systeme Mètrique*, as it appears to be the most consistent. This degree is 57066 toises, equal to 60798 fathoms. But their measurements being all reduced to the temperature of 32° of Fahrenheit's thermometer, the above degree will require a deduction of 19 fathoms nearly, to make it what it would have measured by the brass standard at the temperature of 62°, which is our standard temperature. Hence, 60779 fathoms is the degree in latitude 47° 30′ 46″ to compare with the Indian measurements.

Let this degree be denoted by m'; and its latitude, 47° 30′ 46″ by $'l$; and let the degree 60472,83 fathoms be m, and its latitude 9° 34′ 44″ be l. Then by equation 1,

$e = \frac{m' - m}{3(m.\ \text{Sin.}^2 l' - m'.\ \text{Sin.}^2 l)}$. We shall then have the type of calculation as follows:

$l = 9° 34' 44''$ $\quad m = 60472,83$

$l' = 47\ 30\ 46$ $\quad m' = 60779$

$m' - m = 306,17$

Log. $m = 4,7815603$

log. $(\text{Sin.}^2 l') = \bar{1},7354892$

$4,5169996.$ its nat. no. $= 32885,1$

Log. $m' = 4,7837536$

log. $(\text{Sin.}^2 l) = \bar{2},4423288$

$3,2260824.$ nat no. $1683,0$

$31202,1 = (m\ \text{S.}^2 l' - m'\ \text{S.}^2 l)$

3

Hence $e = \frac{306,17}{93606,3} = \frac{1}{305,73}$ $\quad 93606,3$

Let $l = 13°\ 2'\ 55''$ . . $m = 60487,56$

$l' = 47\ 30\ 46$ . . $m' = 60779$

$291,44 = m' - m$

Log. $m = 4,7816660$

log. $(\text{Sin.}^2 l') = \bar{1},7354892$

$4,5171052$ . . nat. no. $= 32893,2$

Log. $m' = 4,7837536$

log. $(\text{Sin.}^2 l) = \bar{2},7078618$

$3,4911154$ . . nat. no. $= 3098,3$

$29794,9$

Hence $e = \frac{291,44}{89384,7} = \frac{1}{306,70}$ $\quad 3$

$89384,7$

Let $l = 16° 34' 42''$ . . $m = 60512,78$

$'l = 47\ 30\ 46$ . . $m' = 60779$ . .

$m' - m = 266,22$

Log. $m = 4,7818471$

log. (Sin.^{2} $'l$) $= \bar{1},7854392$

$4,5172863$ . . nat. no. $= 32903,8$

Log. $m' = 4,7837586$

log. (Sin.^{2} l) $= \bar{2},9106824$

$3,6944360$ . . nat. no. . . $4948,1$

$27955,7$

8

$83867,1$

Hence $e = \frac{266,22}{83867,1} = \frac{1}{315,03}$

Whence the mean of $\frac{1}{305,73}$; $\frac{1}{306,7}$; $\frac{1}{315,03} = \frac{1}{309,15}$; or the mean compression deduced from the mean degrees given by these three sections, compared with the French measure.

If we proceed in the same manner with the English and Swedish measures, we shall have by the whole as follows:

By the French $\frac{1}{305,73}$; $\frac{1}{306,7}$; $\frac{1}{315,03}$; mean $\frac{1}{309,15}$

By the English $\frac{1}{310,28}$; $\frac{1}{311,36}$; $\frac{1}{318,97}$; mean $\frac{1}{313,54}$

By the Swedish $\frac{1}{305,14}$; $\frac{1}{305,72}$; $\frac{1}{310,72}$; mean $\frac{1}{307,19}$

And the mean of the three means equal $\frac{1}{309,96} = \frac{1}{310,00}$ nearly for the compression at the poles, as deduced from these comparisons; which compression will be adopted for computing the different degrees from the equator to the pole.

All this is supposing the earth to be an ellipsoid. But that

these Indian measurements may rest on their own ground, I shall examine whether the increments to a succession of contiguous degrees as deduced from the present data, be consistent with the elliptic hypothesis, beginning with the degree in latitude 9° 34′ 44″, as determined by observation. To effect this let $m^{(1)}$, $m^{(2)}$, $m^{(3)}$, &c. be the measures of *complete* contiguous degrees on the meridian in latitudes $l^{(1)}$, $l^{(2)}$, $l^{(3)}$, &c. Then, if a meridian of the earth be an ellipse, we know from equation 2, that the compression will be expressed by $\frac{m^{(2)}-m^{(1)}}{3\,(\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)})}$; or $\frac{m^{(3)}-m^{(1)}}{3\,(\text{Sin.}^2 l^{(3)}-\text{Sin.}^2 l^{(1)})}$; or $\frac{m^{(n)}-m^{(1)}}{3\,(\text{Sin.}^2 l^{(n)}-\text{Sin.}^2 l^{(1)})}$; let the length of the diameters be what they will. So that we shall have $\frac{m^{(2)}-m^{(1)}}{3\,m^{(1)}(\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)})}$; $=$ $=\frac{m^{(3)}-m^{(1)}}{3\,m^{(1)}(\text{Sin.}^2 l^{(3)}-\text{Sin.}^2 l^{(1)})}$: or $\frac{m^{(2)}-m^{(1)}}{3\,m^{(1)}(\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)})}$; $=$ $=\frac{m^{(3)}-m^{(1)}}{3\,m^{(1)}(\text{Sin.}^2 l^{(3)}-\text{Sin.}^2 l^{(1)})}$; and by reduction $m^{(3)}-m^{(1)}=$ $=(m^{(2)}-m^{(1)}).\frac{\text{Sin.}^2 l^{(3)}-\text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)}}$

and $m^{(3)}=m^{(1)}+(m^{(2)}-m^{(1)}).\frac{\text{Sin.}^2 l^{(3)}-\text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)}}$

$m^{(4)}=m^{(1)}+(m^{(2)}-m^{(1)}).\frac{\text{Sin.}^2 l^{(4)}-\text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)}}$

$m^{(5)}=m^{(1)}+(m^{(2)}-m^{(1)}).\frac{\text{Sin.}^2 l^{(5)}-\text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)}}$, &c. to

to $m^{(n)}=m^{(1)}+(m^{(2)}-m^{(1)}).\frac{\text{Sin.}^2 l^{(n)}-\text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)}-\text{Sin.}^2 l^{(1)}}$

Also $m^{(2)} = m^{(1)} + (m^{(2)} - m^{(1)}) \cdot \frac{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}}$; that is $m^{(2)} = m^{(1)} + (m^{(2)} - m^{(1)})$, by preserving the expression $m^{(2)} - m^{(1)}$, which we will call d. Then we shall have

$$m^{(1)} = m^{(1)} + o$$

$$m^{(2)} = m^{(1)} + d$$

$$m^{(3)} = m^{(1)} + d \left\{ \frac{\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}} \right\}$$

$$m^{(4)} = m^{(1)} + d \left\{ \frac{\text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}} \right\}, \text{ \&c.}$$

$$\text{to } m^{(n)} = m^{(1)} + d \left\{ \frac{\text{n.}^2 l^{(n)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}} \right\}$$

Here d is the only unknown quantity to be determined, since $m^{(1)} + m^{(2)} + m^{(3)} \ldots m^{(n)} = A$. the terrestrial measure of the arc of n complete degrees; $m^{(1)}$ being the measure of the first degree in latitude $l^{(1)}$ by observation.

Then $A = nm^{(1)} + d \left\{ o + 1 + \frac{\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)} \ldots \text{Sin.}^2 l^{(n)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}} \right\}$

And $d = \left\{ \frac{(A - nm^{(1)}) \cdot (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})}{(\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}) + \ldots \text{Sin.}^2 l^{(n)} - \text{Sin.}^2 l^{(1)}} \right\}$

when d becomes a known quantity. And since $\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}$ is a constant and known quantity, if $\frac{d}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}}$ be called Q, we shall have the order of contiguous degrees as follows:

$$m^{(1)} = m + o$$

$$m^{(2)} = m + d$$

$$m^{(3)} = m + Q \left\{ \text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)} \right\}$$

$$m^{(4)} = m + Q\left\{\text{Sin.}^2 l^{4)'} - \text{Sin.}^2 l^{(1)}\right\} \text{ \&c.}$$
$$\text{to } m^{(n)} = m + Q\left\{\text{Sin.}^2 l^{(n)} - \text{Sin.}^2 l^{(1)}\right\}$$

To apply this formula to the present measurement, it will be necessary to have a terrestrial arc to correspond with the celestial one of complete degrees, and the first degree determined by observation. If we begin with the degree in latitude 9° 34′ 44″, which is 60472,83 fathoms, as the mean degree deduced from an arc of 2° 50′ 10″, 54, where the

	fathoms.
corresponding terrestrial arc, is - -	171516,75
The half of which is the distance of the middle point of the arc from Patchipolliam, equal -	85758,375
To which add half the degree south, or -	30236,415
Their sum is the terrestrial arc between half the degree south of the middle point and Patchipolliam, - - - =	115994,790

The latitude of whose commencement is 9°4′33″,66 the latitude of the south extremity of an arc of complete degrees.

Now the terrestrial arc between Patchipolliam and Namthabad is - - -	248188,534
And between Namthabad and Daumergidda -	178904,700
Their sum is the terrestrial arc between 9° 34′ 43″ 66 and Daumergidda - -	543088,024
The latitude of Daumergidda, by adding the arc between Namthabad and Daumergidda (=2° 57′ 23″,32) by 13 stars, to the latitude of Namthabad (15° 6′ 0″,21) gives -	18°03′ 23″,64
The latitude of Daumergidda, by adding the whole arc between Punnæ and Daumergidda	

($9^\circ\ 53'\ 45'',25$) as determined by seven corresponding stars, to the latitude of Punnæ, is $18^\circ\ 03'\ 23'',64$

The mean of which, or correct latitude, is $18\ 03\ 23\ ,58$

Hence from $18^\circ\ 3'\ 23'',58$

Subtract $9\ 4\ 43\ ,66$

Dif. or arc $8\ 58\ 39\ ,92$ whose measure is 543088,024

To which add $0\ 1\ 20\ ,08$ whose measure is 1345,184

Gives the N°. n of complete degrees $= 9^\circ\ 0'0''$ whose measure (A) is 544433,21

Now the measure of the first degree is - 60472,83

And $n = 9$, therefore $n\,m^{(1)}$ is equal - 544255,47

Which subtracted from A gives $A - nm^{(1)}$ = 117,74

And $\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)} = ,006014$; therefore, $006014 \times 117,74 = 1,0689284 = (A - n\,m^{(1)}) \cdot (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})$ the numerator; and the denominator $(\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(2)}) + \ldots (\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)})$ is equal 0,263137. Hence we shall have $\frac{(A - nm^{(1)}) \cdot (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})}{(\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}) + \ldots (\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)})}$, equal $\frac{1,0689284}{0,263137} = 4,06225 = d$; and $\frac{d}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}} = \frac{4,06225}{,006014} = 675,47 = Q$, and from these the following table has been constructed.

TABLE I.

	Degrees.	Latitudes.
$m^{(1)} = m^{(1)} + o$ - - -	60472,83	° 9 ′ 34 ″ 44
$m^{(2)} = m^{(1)} + d$ - - -	60476,89	10 34 44
$m^{(3)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})$	60481,34	11 34 44
$m^{(4)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(1)})$	60486,16	12 34 44
$m^{(5)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(5)} - \text{Sin.}^2 l^{(1)})$	60491,36	13 34 44
$m^{(6)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(6)} - \text{Sin.}^2 l^{(1)})$	60496,92	14 34 44
$m^{(7)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(7)} - \text{Sin.}^2 l^{(1)})$	60502,85	15 34 44
$m^{(8)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(8)} - \text{Sin.}^2 l^{(1)})$	60509,12	16 34 44
$m^{(9)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)})$	60515,74	17 34 44
Sum -	544438,21	= A

According to this table, the degree in latitude 16° 34′ 44″ is 60509,12 fathoms; and the mean degree for latitude 16° 34′ 42″, as deduced from the arc between Namthabad and Daumergidda is 60512,78 fathoms, which exceeds the computed one for latitude 16° 34′ 44″ (which may be considered the same) only 3,66 fathoms.

It may however be necessary to notice that any one of the expressions $\frac{m^{(2)} - m^{(1)}}{3\,(\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})}$; $\frac{m^{(3)} - m^{(1)}}{3\,(\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})}$, &c. will bring out a compression equal $\frac{1}{289}$ nearly, which differs considerably from the general mean. But a very small difference in the numerator will produce a great difference in the compression.

If we suppose $\frac{1}{310}$ to be the true compression, let it be determined what the value of $m^{(1)}$ ought to be to bring out that compression; and by that means to detect the errors of the observed degrees, in latitude 9° 34′ 44″, and 16° 34′ 42″, which last may be compared with $m^{(8)}$.

Put $A = 544483,21$; $a = \text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)} = ,006014$, radius being unity; $b = (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}) + \ldots\ldots (\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)}) = ,263137$.

Then $d = (m^{(2)} - m^{(1)}) = \frac{(A - nm^{(1)}).a}{b}$; and $\frac{d}{3m^{(1)}.a} = \frac{A - nm^{(1)}}{3b.m^{(1)}} = \frac{1}{310}$; from which is deduced $m^{(1)} = \frac{310.A}{3b + 310.n} = 60475,47$ fathoms. Whence $d = \frac{(A - nm^{(1)}).,006014}{,263137} = 3,5192$. And $Q = \frac{d}{,006014} = 585,17$. From these the following table has been computed, from which it appears that the first degree by measurement, is 2,6 fathoms in defect; and that the one in latitude 16° 34′ 42″ is 5,89 fathoms in excess; either of which is too small to affect the elliptic hypothesis; the greatest being only $\frac{1}{5}$ of a second on the earth's surface.

TABLE II.

	Degrees.	Latitudes. ° ′ ″
$m^{(1)} = m^{(1)} + o$ - -	60475,47	9 34 44
$m^{(2)} = m^{(1)} + d$ - - -	60478,99	10 34 44
$m^{(3)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})$	60482,84	11 34 44
$m^{(4)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(1)})$	60487,02	12 34 44
$m^{(5)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(5)} - \text{Sin.}^2 l^{(1)})$	60491,58	13 34 44
$m^{(6)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(6)} - \text{Sin.}^2 l^{(1)})$	60496,34	14 34 44
$m^{(7)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(7)} - \text{Sin.}^2 l^{(1)})$	60501,47	15 34 44
$m^{(8)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(8)} - \text{Sin.}^2 l^{(1)})$	60506,91	16 34 44
$m^{(9)} = m^{(1)} + Q\,(\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)})$	60512,64	17 34 44
Sum	544433,91	= A

From inspecting these two tables, it appears that the degree in latitude 13° 34′ 44″ is nearly the same in each, and the mean is 60491,46 fathoms; which certainly must be near the truth. I shall therefore adopt it with the compression $\frac{1}{310}$ for computing the general tables of degrees for every third degree of latitude from the equator to the pole.

With respect, however, to the compression, that nothing may be left undone to give full and entire satisfaction on this subject, I shall here add an investigation similar to that given by Professor PLAYFAIR, in the 5th vol. of the Edinburgh Philosophical Transactions, where, in place of using the measures of degrees due to particular latitudes, two measured arcs of large amplitudes are made use of, the latitudes of whose extremities have been determined with great accuracy.

Let ADBE be a meridian of the earth, where A is at the equator, and D at the pole. Suppose F to be any point on that meridian, and FH the radius of curvature of the ellipse at that point. Put A C $= a$; DC $= b$; c being the centre of the ellipse; and let A be equal the angle AKF, the latitude of F; or let it be the measure of the arc of latitude aKf, to radius unity, or aK: that is, the *measure* of the angle aKf in parts of the radius aK, or unity. Let GF be an indefinitely small part of the ellipse. Then if AF $= x$, GH $= \dot{x}$ the fluxion of the arc AF of the ellipse; and if GH be drawn, then the angle GHF $= <g$K$f = \dot{A}$ or fg the fluxion of the arc of latitude af to radius 1. Hence as $1 : \dot{A} :: FH : \dot{x} = \dot{A}.FH$. But the radius of curvature FH $= a^2 b^2 (a^2 - a^2.$ Sin.^{2} A $+ b^2$ Sin. A$)^{-\frac{3}{2}}$. Let e be the ellipticity, or $a - b$; then $b = a - e$, and $b^2 = a^2 - 2ae$ very nearly, since e^2 is very small. Hence FH $= a^2 (a - 2e) . (a^2 - 2ae.$ Sin.^{2} A$)^{-\frac{3}{2}}$. But $(a^2 - 2ae.$ Sin.^{2} A$)^{-\frac{3}{2}} = (a^2)^{-\frac{3}{2}} . (1 - \frac{3e}{a}.$ Sin.^{2} A$)^{-\frac{3}{2}} =$ $= a^{-3} . (1 + \frac{3e}{a}.$ Sin.^{2} A) very nearly, by rejecting all the terms involving e^2 and its higher powers. Hence FH $=$ $= a^3 (a - 2e) . a^{-3} . (1 + \frac{3e}{a}.$ Sin.^{2} A$) = a - 2e + 3e.$ Sin.^{2} A, which substituted for FH, we get $\dot{x} = \dot{A}(a - 2e + 3e.$ Sin.^{2}A$) =$ $= \dot{A} (a - 2e) + \dot{A} (3e.$ Sin.^{2} A$)$. But Sin.^{2} A $= \frac{1 - \text{Cos. } 2A}{2}$, and therefore $\dot{x} = \dot{A} (a - 2e) + \frac{3}{2} e \dot{A} - \frac{3}{2} e \dot{A}.$ Cos. 2 A; whose fluent is $x = (a - \frac{3}{2}e)$A $+ \frac{3}{4}e.$ Sin. 2A $= a$A $- e (\frac{A}{2} + \frac{3}{4}$ A.Sin.2A) which requires no correction. And this is the measure of an

arc on the meridian extending from the equator to the latitude of the point F; where A denotes the arc of latitude in parts of the radius 1.

Let F′ be any other point on the meridian, whose arc of latitude is A′. Then $AF' = a\,A' - e\left(\frac{A'}{2} + \frac{1}{4}\,A'.\ \text{Sin. } 2A\right)$ and therefore $F\,F' = a(A' - A) - e\left\{\frac{A' - A}{2} + \frac{1}{4}.\ \text{Sin. } 2A' - \frac{1}{4}.\ \text{Sin. } 2A.\right\}$

Let F″, F‴, be any other two points on the meridian whose respective arcs of latitude are A″ and A‴. Then from the same reasoning as above, we have $F''\,F''' = a\,(A''' - A'') - e\left\{\frac{A''' - A''}{2} + \frac{1}{4}.\ \text{Sin. } 2\,A''' - \frac{1}{4}\ \text{Sin.}^2\,A''\right\}$

Now FF′ and F″ F‴ are here supposed to be measured arcs on the meridian, whose respective lengths in fathoms may be called L and L′, corresponding with the celestial arcs A′—A, and A‴ — A″. To shorten the operation, put A′— A = r; A‴ — A″ = r'. Also $\frac{A' - A}{2} + \frac{1}{4}\ \text{Sin. } A' - \frac{1}{4}.\ \text{Sin. } A = S$, and $\frac{A''' - A''}{2} + \frac{1}{4}.\ \text{Sin. } 2\,A''' - \frac{1}{4}.\ \text{Sin. } 2\,A'' = S'$. Then we have $L = ar - es$; $L' = ar' - es'$. And therefore $a = \frac{s'L - sL'}{rs' - r's}$; — $e = \frac{r'L - rL'}{rs' - r's}$; and $\frac{e}{a} = \frac{r'L - rL'}{s'L - sL'}$ equal the compression expressed in fractional parts of the semi-equatorial diameter.

To apply this to the case in question,

Let A = the latitude of Punnæ	=	8°	9′	38″,4
A = the lat. of Daumergidda	=	18	8	23,6
A′ — A = r	-	9	58	45,2 = ,1727158
A″ = lat. of Montjouy	-	41	21	44,96
A‴ = lat. of Dunkirk	-	51	02	09,2
A‴ — A″ = r'	-	9	40	24,24 = ,1688927

Put $s = \frac{A' - A}{2} + \frac{3}{4}$ Sin. 2 A′ $- \frac{3}{4}$. Sin. 2 A $=$, 3176258

$s' = \frac{A'' - A'}{2} + \frac{3}{4}$ Sin. 2 A″ $- \frac{3}{4}$. Sin. 2 A″ $=$, 0738689

L $=$ 598610 } arc between { Punnæ and Daumergidda,

*L′ $=$ 587475,41 } arc between { Montjouy and Dunkirk.

$a = \frac{s'L - sL'}{rs' - r's} = 3483955$

$e = \frac{r'L - rL'}{rs' - r's} = 9820,8$

$\frac{e}{a} = \frac{r'L - rL'}{s'L - sL'} = \frac{1}{355}$ nearly.

In the paper which I sent to the Asiatick Society, and which will appear in the 13th volume of their Researches, the terrestrial arc between Barcelona and Dunkirk, as given in the 2d volume of Colonel MUDGE's Survey, was made use of, and is there stated to be 587987 fathoms, which gives the compression by this method $\frac{1}{272}$. But there must be some mistake in this; for by comparing it with the distance between Montjouy and Dunkirk, as given by DE LAMBRE, the former is considerably greater than the latter, though Montjouy is 3″ south of Barcelona. The mean degree for latitude 47° 24′ used in that paper for determining the ellipticity, compared with the Indian measurements, was deduced from that arc, and gave the compression $\frac{1}{291,6}$, while the general mean compression obtained by comparing these measurements with the French, English, and Swedish degrees, was $\frac{1}{304}$ nearly.

Since it is here determined to adopt $\frac{1}{310}$ as the compression, and 60491,46 fathoms for the measure of the degree

* See vol. iii. p. 89. Base du Systeme Métrique, where the arc between Montjouy and Dunkirk is 551583,6 toises, or 587657,17 fathoms, at the temperature of 32°, which reduced to the temperature of 62°, will be 587475,41 fathoms.

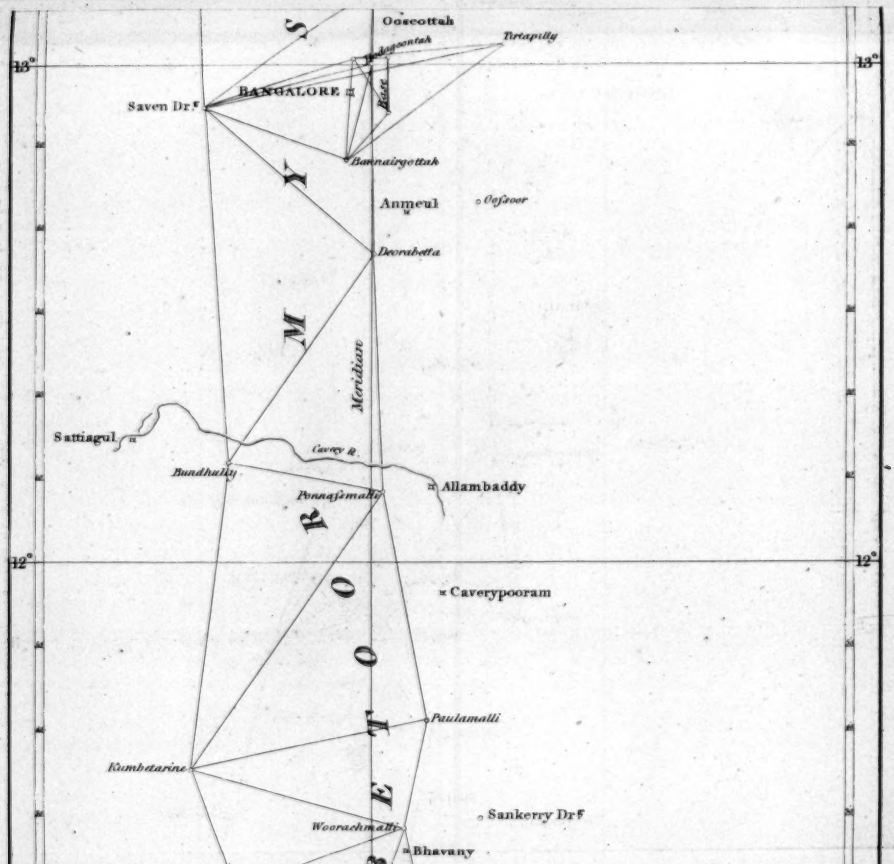
on the meridian, due to latitude 13° 34′ 44″; we shall have $m' = 60491{,}46$; $l' = 13°\ 34'\ 44''$. Then if m be the degree in any other latitude l; by equation 5, $m = \frac{1 + 3e.\ \text{Sin.}^2 l}{1 + 3e.\ \text{Sin.}^2 l'} . m'$. If m have its middle point on the equator, where $l = 0$, then $m = \frac{m'}{1 + 3e.\ \text{Sin.}^2 l} = \frac{60491{,}46}{1{,}00053345} = 60459{,}2$ fathoms.

By equation 16, $p = m . \frac{1 + 3e'}{1 + e} = 60459{,}2 . \frac{1 + 3e'}{1 + e} =$ $= 60459{,}2 \times 1{,}006431 = 60848$ fathoms for the degree on the equatorial circle. Put $A = 57°{,}2957795$ the arc equal radius. Then $A . p = 57°$, &c. $\times\ 60848 = 3486334 = \frac{1}{2}a$; and therefore $a = 6972668$ fathoms; and consequently $b (= a . (1 - e) = 6972668 \times 9967742 = 6950176$ fathoms, the length of the polar axis. Now since 6972668 is the diameter of the equatorial circle, then 3,14159, &c. multiplied by 6972668, gives 21905280 fathoms for the circumference of the circumscribing the elliptic meridian. Put $d = 1 - \frac{b^2}{a^2} =$, 00644. Then as $1 : 1 - (\frac{d}{2^2} + \frac{3d^2}{2^2 . 4^2}$ &c.) : : 21905280, the circumference of the circumscribing circle : 21869976 = the circumference of the elliptic meridian; which, divided by 4, gives 5467494 fathoms for the quadrantal arc of that meridian; and this reduced into inches, and divided by 10,000000, will give 39.366 inches for the French mètre, at the temperature of 62°. Now, the mètre deduced from the measurements of DE LAMBRE and MECHAIN, and reduced from 32° to 62°, was 39,371, English inches, which exceeds this one by ,005 inches: a quantity too small to affect any standard measure: so that the mètre as deduced from a comparison of all the recent operations, may be considered, as to practical purposes, the same as that which has been

adopted by the French mathematicians, being obtained from comparing the measurements of De Lambre and Mechain, with those of Bouguer and Condamine.

As I am in hopes that another section, and perhaps more, will be added to the arc, I shall defer making any final conclusions till I see what may be done. The next station of observation, I propose to be as near the latitude of 21° 6′ 5″ as possible, in order that the middle point of the section may fall in 19° 34′ 44″, so as to compare the mean degree as obtained by observation, with the one computed from the increments as in the foregoing tables. I say another section, and *perhaps more*; because should the country to the northward be open and settled, there may be a possibility at some future day, of continuing the same arc to the northern confines of Hindostan: so much, at least, seems necessary for laying the foundation of Indian geography; and if it were conducted with zeal and judgment, it would not be a work of many years, provided the features of the country be favourable. The whole time taken up in the measurement of the arc between Punnæ and Daumergidda, including the base lines, astronomical observations, &c.; that is to say, the entire field work, has only been three years and nine months; and a considerable part of the corrections for the stars, for the angles, and for the reduction of the base, were done during the time of measuring the base and observing for the zenith distances; so that I suppose *four years and a half* may be allowed for the whole work. From this estimate, the meridional arc might be continued from Daumergidda to Dhelli in about five years, if no local impediment disturbed its progress. It is however probable that difficulties might occur in Sindia's country,

which a northern direction from Daumergidda would render it necessary to pass through. But it would be a sufficient point gained, if a series of triangles were carried from Nagpoor in Berar, to Kalpy on the Jumna, which two places, if the maps are correct, lie nearly on the same meridian. From Kalpy, the meridional series might perhaps be continued north to the Kemaoon mountains; Kalpy would also be a favourable position from which to extend a series to the east and west, and for meridian stations not more than sixty or seventy miles from each other, where the positions of the meridians ought to be determined by pole star observations. Data would then be had for extending the survey on a more enlarged scale over the upper provinces; and the arc between Nagpoor and Kalpy might be easily reduced to the one terminating in latitude 21° 6′ (that station of observation and the one near Nagpoor being connected), so as to form one entire arc between the parallels of Punnæ and Kalpy. Thus would be formed a geometrical connection between the southern and northern possessions of the East India company, and a complete basis laid for local and detailed surveys of the whole. Innumerable might be the individuals employed in carrying them into effect, and various might be the description of these surveys. By the assistance of this work as a foundation, they might all be rendered useful; but without it, no combination of the best common surveys could ever be formed into a correct map to embrace such an extensive territory as British India.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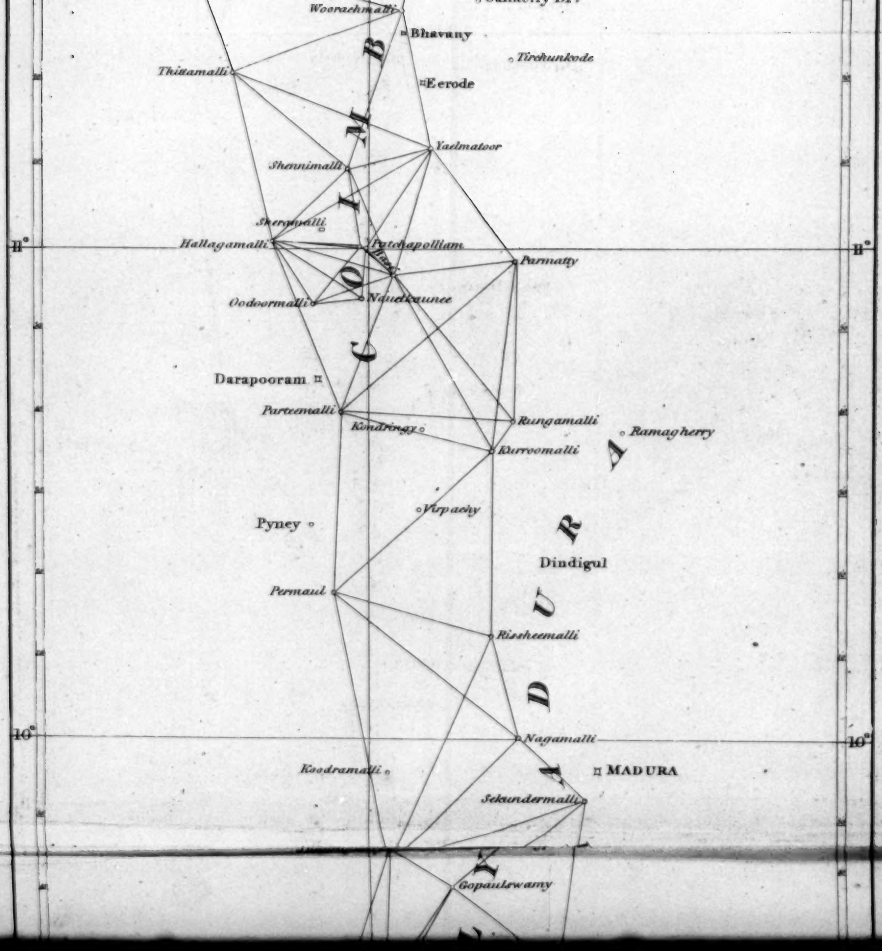
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Darapooram
Parteemalli
Rungamalli
Kondrengy
Ramagherry
Kurroomalli
Virpachy
Pyney
Dindigul
Permaul
Rissheemalli
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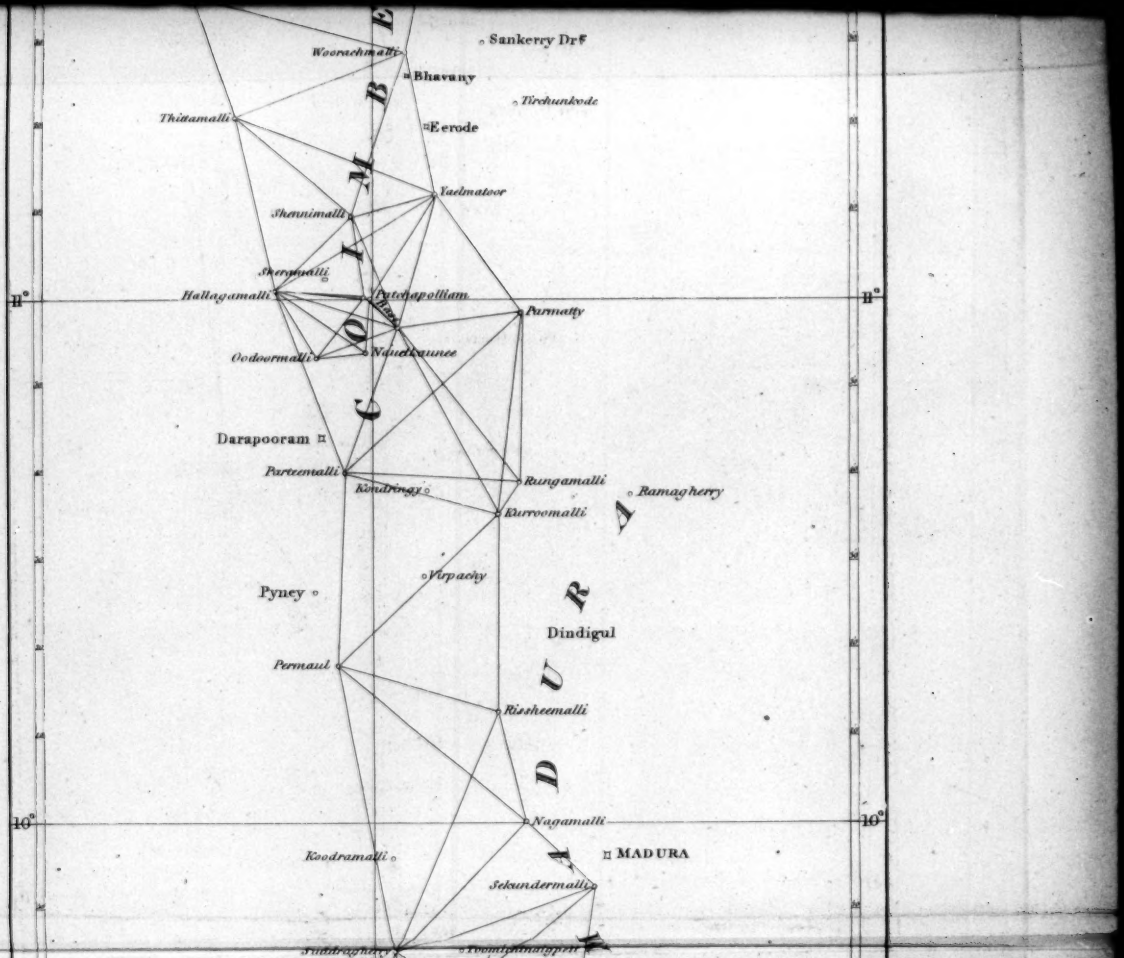
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Oodoormalli
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Darapooram
Partemalli
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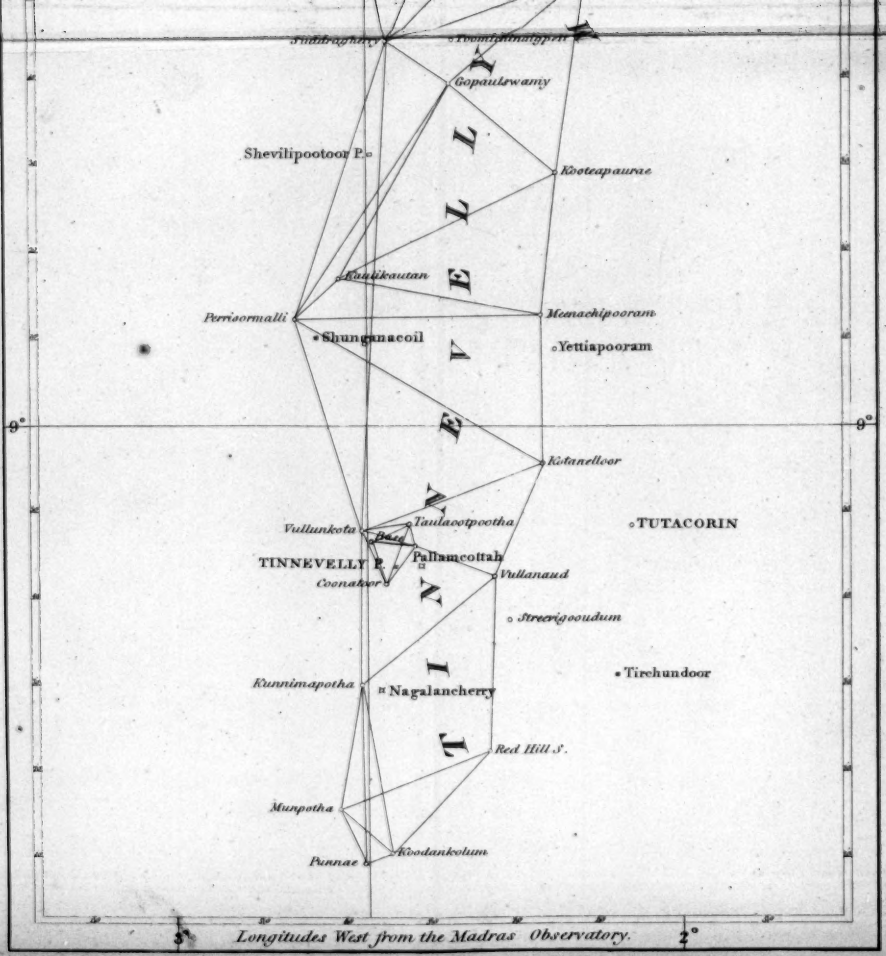
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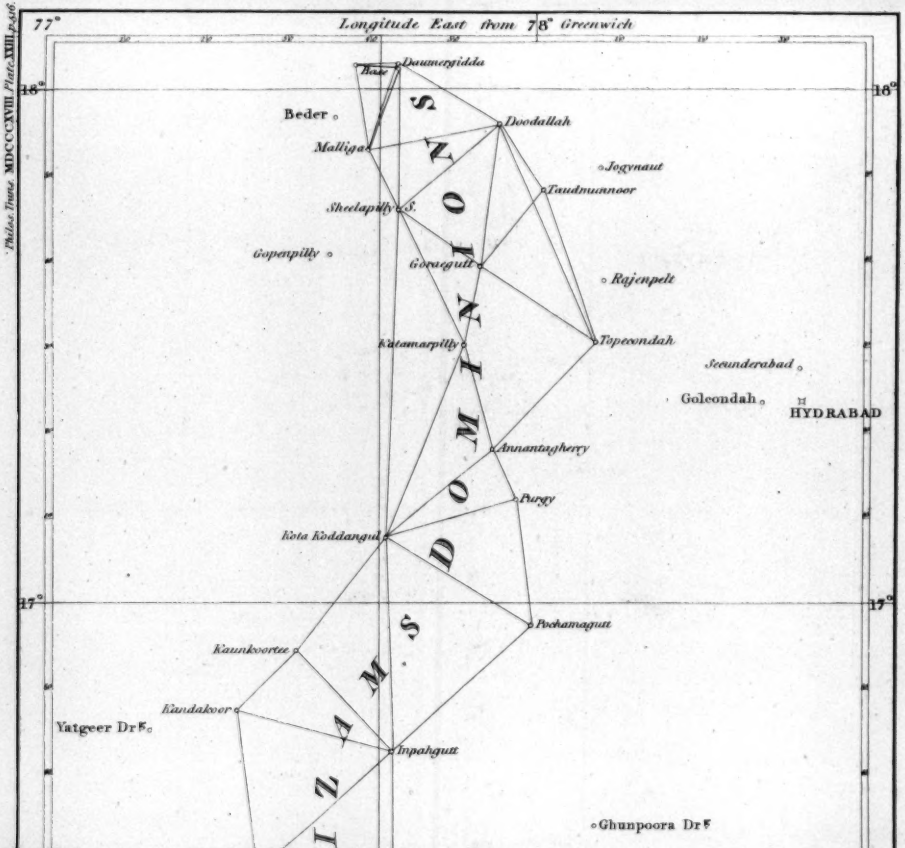
Vullunkota
Taulacotpootha
TUTACORIN
Beed
TINNEVELLY P.
Pallamcottah
Coonakier
Vullanaud
Streerigoodum
Tirchundoor
Kunnimapotha
Nagalancherry
Red Hill S.
Munpotha
Punnae
Koodankolum
T I N N E V E L L Y
3°
Longitudes West from the Madras Observatory.
2°


77°
Longitude East from Greenwich
78°
18°
17°
Base
Daumergidda
Beder
Doodallah
Malliga
Jogynaut
Taudmunnoor
Sheelapilly
S.
Gopenpilly
Goreagutt
Rajenpelt
Kutamarpilly
Topecondah
Secunderabad
Golcondah
HYDRABAD
Annantagherry
Purgy
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Ghunpoora Dr.
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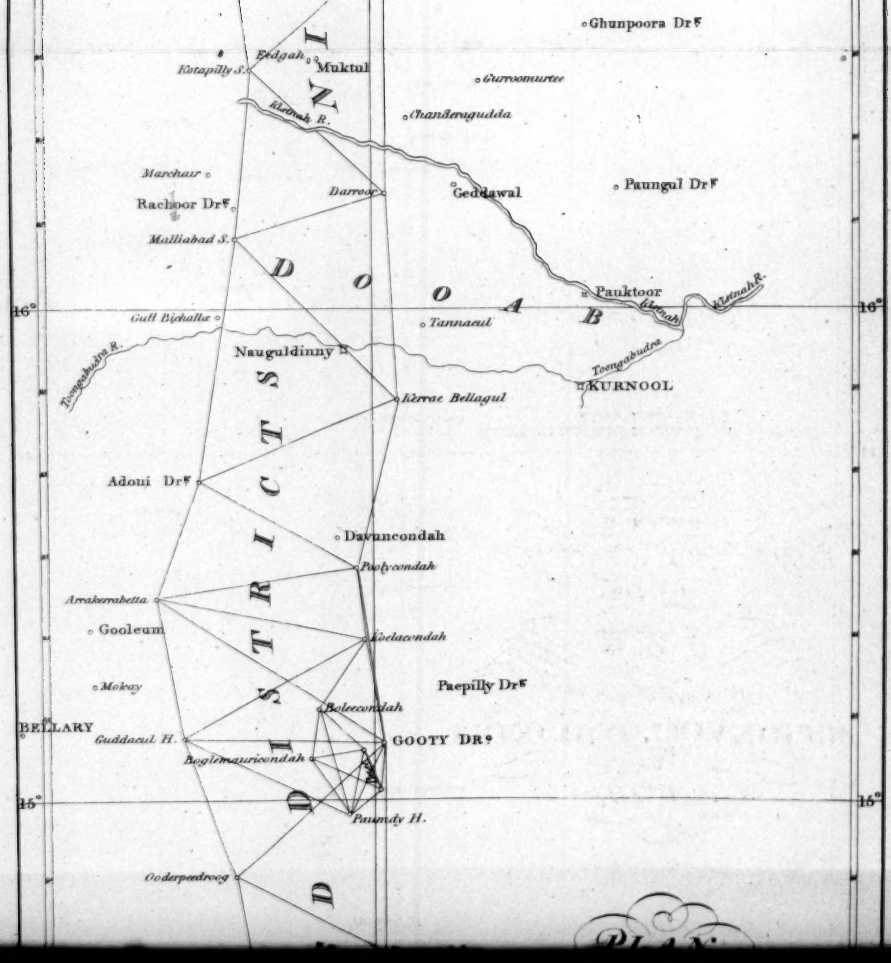
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Geddawal
Paungul Drs
Malliabad S.
DOOAB
Pauktoor
Kistnah R.
16°
Gulf Bichalla
Tannaeul
Toongabudra R.
Nauguldinny
Toongabudra
KURNOOL
Kerrae Bellagul
DISTRICTS
Adoni Drs
Davuncondah
Pooloocondah
Arrakerrabetta
Gooleum
Kudacondah
Moloay
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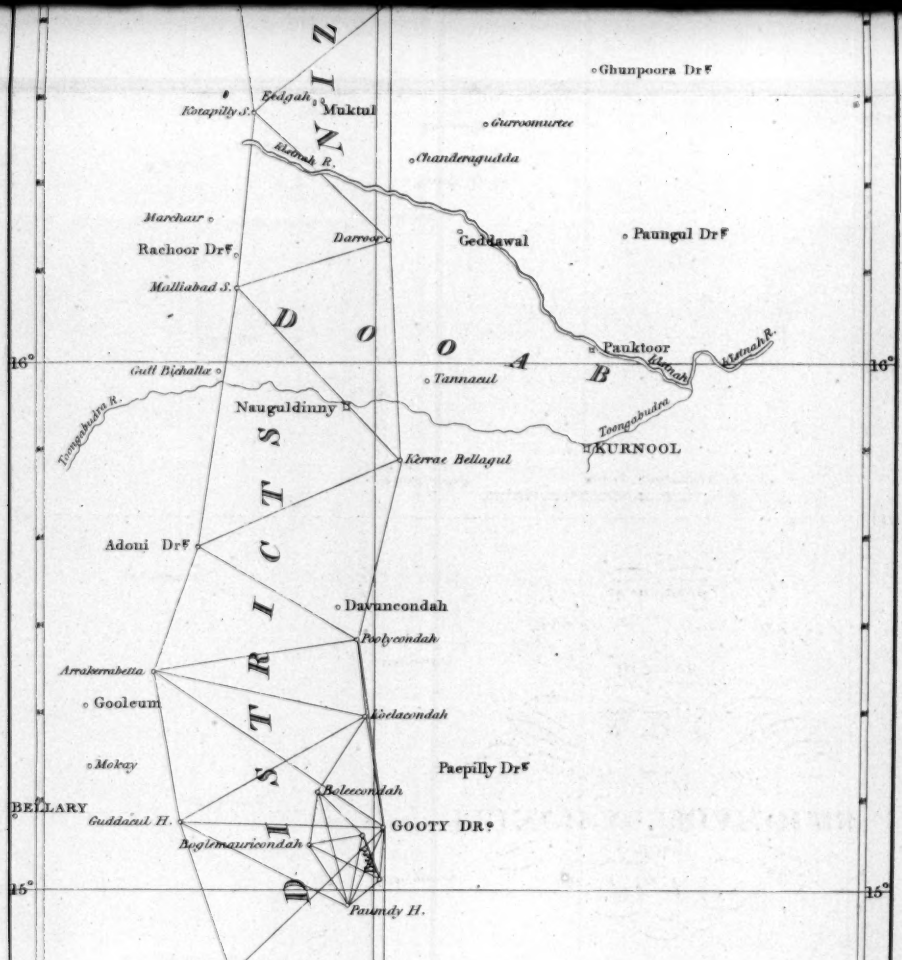
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Geddawal
Paungul Dr.
Malliabad S.
DOAB
Pauktoor
Kistnah R.
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DISTRICTS
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Arrakurrabetta
Gooleum
Koolacondah
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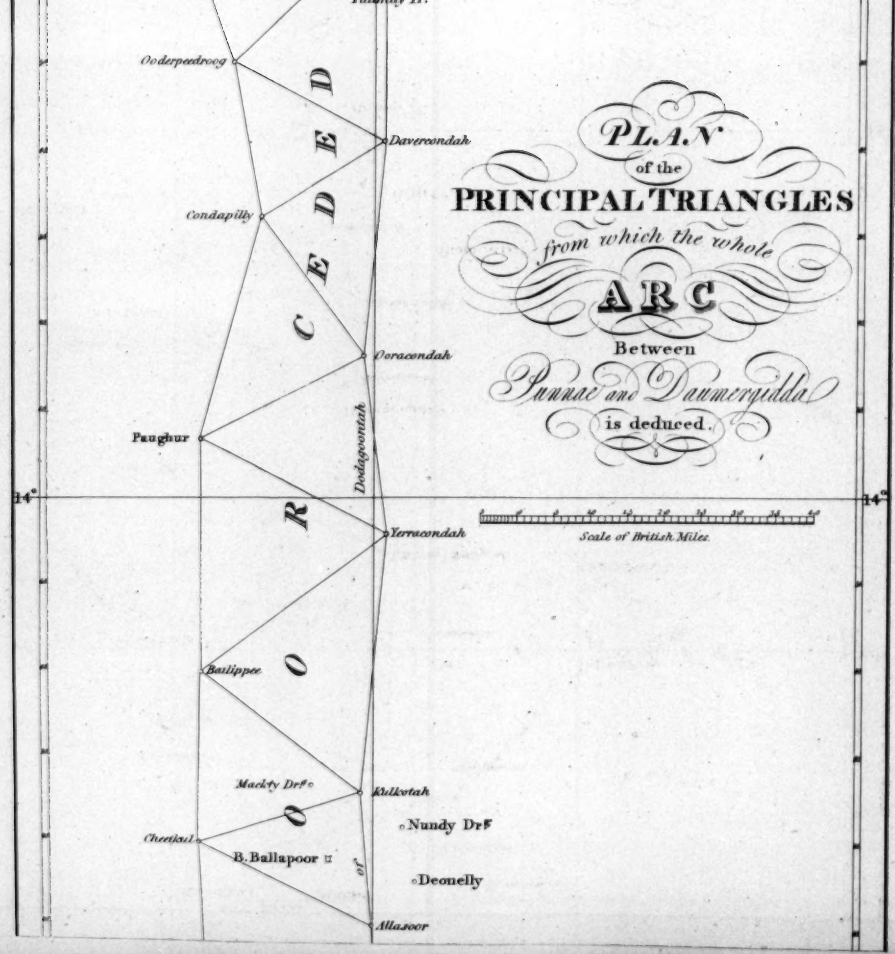
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from which the whole
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is deduced.
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Davercondah
Condapilly
Ooracondah
Paughur
Dodagoontah
14°
14°
Yerracondah
Scale of British Miles.
Ballippee
Mackty Drg
Kilkotah
Nundy Drg
Cheekael
B. Ballapoor
Deonelly
Allasoor
DECCAN
CORD

The lengths of different degrees computed from the foregoing data, for every three degrees from the equator to the pole.

Lat.	Degrees on the meridian.	Degrees on the perpendicular.	Degrees of Longitude.
0	60459,2	60848,0	60848,0
3	60460,8	60848,4	60765,0
6	60465,6	60850,1	60516,8
9	60473,5	60852,8	60103,6
12	60484,5	60856,5	59526,7
15	60498,4	60861,1	58787,3
18	60515,1	60866,7	57887,7
21	60534,3	60873,2	56830,0
24	60556,0	60880,5	55628,1
27	60579,8	60888,5	54252,0
30	60605,5	60897,1	52738,4
33	60632,7	60906,2	51080,2
36	60661,3	60915,8	49281,9
39	60690,8	60925,7	47348,2
42	60721,3	60935,7	45284,0
45	60751,8	60946,1	43095,4
48	60782,3	60956,4	40787,8
51	60812,5	60966,5	38367,5
54	60842,1	60976,5	35841,1
57	60870,7	60986,1	33215,4
60	60858,0	60995,2	30497,6
63	60923,7	61003,8	27695,2
66	60947,5	61011,8	24815,7
69	60969,1	61018,9	21867,2
72	60988,3	61025,0	18857,9
75	61005,1	61031,0	15796,0
78	61018,9	61035,8	12690,1
81	61029,9	61039,5	9548,7
84	61037,8	61042,1	6380,6
87	61042,6	61043,7	3194,8
90	61044,3	61044,3	—

PRESENTS

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	Commentationes Societatis Regiæ Scientiarum Gottingensis Recentiores. Vol. III. ad A. MDCCCXIV-XV. Gottingæ, 1816. 4°	The Royal Society of Sciences of Gottingen.
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	A Treatise on the Nature and Cure of Gout and Rheumatism, by Charles Scudamore, M. D. London, 1817. 8°	Dr. Charles Scudamore.
	An Essay on the Nature of Heat, Light, and Electricity, by Charles Carpenter Bompass. London 1817. 8°	Charles Carpenter Bompass, Esq.
	Medico-Chirurgical Transactions. Vol. VIII. pt. I. London, 1817. 8°	The Medico-Chirurgical Society.
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	Canon Pellianus, sive Tabula simplissimam Æquationis celebratisimæ $y^n = ax^n + 1$ Solutionem pro singulis numeri dati valoribus ab 1 usque ad 1000 in numeris rationalibus iisdemque integris exhibens. Auct. Car. Ferd. Degen. Hauniæ, 1817. 12°	Car. Ferd. Degen.
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	The Philosophical Magazine, No. 231 to 234.	Mr. A. Tilloch.
	The European Magazine from July to Oct. 1817.	The Editor.
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	Dissertatio Botanica de Dracæna.	Professor C. P. Thunberg.
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Dec. 11.	Inaugural Address delivered in the Chapel of the University of Cambridge, Dec. 11, 1816, by Jacob Bigelow, M. D. &c. Boston, 1817. 8°	The Corporation of the University of Cambridge, United States.
	Annals of Philosophy, No. 60.	Dr. Thomas Thomson.
	Philosophical Magazine, No. 235.	Mr. Alexander Tilloch.
	Monthly Review for November.	Mr. G. E. Griffiths.
	European Magazine for November.	The Editor.
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Jan. 8.	Quomodo ex Observatione Occultationis Stellæ fixæ a Lunâ effectæ Longitudo Geographica Loci observationis computetur explicavit Frid. Guil. Toennies. Berolini, 1816. 4°	Frid. Guil Toennies.

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	Librorum impressorum qui in Museo Britannico adservantur Catalogus. Lond. 1813, &c. Vols. I.-VI. 8°	The Trustees of the British Museum.
	Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society, Vol. VIII. London, 1817. 8°	The Medical and Chirurgical Society.
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	The Philosophical Magazine, No. 236.	Mr. A. Tilloch.
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	The European Magazine for December.	The Editor.
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	Report of a Committee of the Linnean Society of New England relative to a large Marine Animal, supposed to be a Serpent, seen near Cape Ann, Massachusetts, in August, 1817. Boston, 1817. 8°	The Linnean Society of New England.

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Feb. 5.	Astronomische Beobachtungen auf der Koniglichen Universitäts Sternwarte in Konigsberg von F. W. Bessel, zweite Abtheilung vom 1 Januar bis 31 Decem. 1815. Konigsberg, 1818. fol.	Professor Bessel.
	Annals of Philosophy, No. 62.	The Editors.
	The Philosophical Magazine, No. 237.	Mr. A. Tilloch.
	The Monthly Review for January, and Appendix to Vol. LXXXIV.	Mr. G. E. Griffiths.
	The European Magazine for January.	The Editor.
12.	Annales des Mines, Vol. I. and Nos. 1, 2, 3, of Vol. II.	Le Conseil Général des Mines de France.
	Surgical Essays by Astley Cooper and Benjamin Travers, Part I. London, 1818. 8°	Astley Cooper and Benj. Travers, Esq.
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	The European Magazine for February.	The Editor.
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	Verbesserung der Luftschiffahrt von Erasmus Lennig, Mainz 1818. 12°	Erasmus Lennig.
April 2.	Annals of Philosophy, No. 64.	The Editors.
	The Philosophical Magazine, No. 239.	Mr. Alex. Tilloch.
	The Monthly Review for March.	Mr. G. E. Griffiths.

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	The Philosophical Magazine, No. 240.	Mr. Alexander Tilloch.
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	The European Magazine for April.	The Editor.
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	Additional Bye Laws of the Horticultural Society of London. London, 1817. 4°	———
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	Shums-ool-Loghat, or a Dictionary of the Persian and Arabic Languages, the Interpretation being in Persian, comprising also such words of the Turkish Language as occur in the works of Persian and Arabic Authors, compiled from original Dictionaries of authority in those languages by learned Natives, under the inspection of Joseph Barretto, junior. Calcutta, 1806, 2 Vols. 4°	J. Barretto, junior, Esq.
	A Dictionary of the Persian and Arabic Languages, by Joseph Barretto, junior. Calcutta, 1804. 2 Volumes. 8°	———
	Astronomical Observations made at the Radcliffe Observatory at Oxford from May 1, 1817, to May 1, 1818, by the Rev. Abram Robertson, D. D. &c. Fol. MS.	The Trustees under the will of the late Dr. Radcliffe.
	Transactions of the American Philosophical Society held at Philadelphia for promoting useful Knowledge. Vol. I. New Series. Philadelphia, 1818. 4°	The American Philosophical Society.
	Essai Sur l'Ordre considéré dans l'Administration publique et dans les Sciences, par Auguste Jullien, &c. Paris, 1818. 8°	M. A. Jullien.
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